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Illustrated with Engravings.

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BY WILLIAM NICHOLSON.

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## ADVERTISEMENT.

THE Authors of Original Papers in the present Volume, are Joseph Huddart, Esq. F. R. S.; John Dalton; Everard Home, Esq. F. R. S.; Mr. John Gough; Robert Briggs, M. D.; Mr. Ezekiel Walker; Mr. John Farcy; Mr. Frederick Accum; Sir A. N. Edelcrantz; Richard Chenevix, Esq. F. R. S. and M. R. I. A.; Mr. R. Winter; Mr. James Stodart; Mr. T. Jones; W. H. Woollaston, M. D. F. R. S.; J. M. Elliott; Mr. William Close; Mr. W. Jones, F. Am. P. S.; C. Wilkinson, Esq.; Mr. J. C. Hornblower; Mr. J. J. Hawkins; I. R. I.; Mr. Sharples; B.

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Of English Works, abridged or extracted; Rev. Richard Yates, F. R. S.; Mr. Tho. Willis; John Playfair, F. R. S.; S. H. C. Englefield, Bart. F. R. S.; William Herschel, L. L. D. F. R. S.; T. S. Dyot Bucknal, Esq. M. P.; Benjamin Smith Barton, M. D.; John Stockwell, Esq.; Mr. John Cowie.

Of Engravings the Subjects are, 1. Station Pointer for determining the precise Situation of a Ship or Vessel from two Horizontal Angles, by Joseph Huddart, Esq. F. R. S. 2. Mr. Dalton's Scheme for shewing the Distribution of the Particles in Mixed Gases. 3. Diagram to shew the Method of constructing and computing the Situation of an Observer from

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from the Angular Position of Three known Objects. 4. Double Barrel and Winch for encreasing the Power without Diminution of Strength. 5. Screw Press, which by the contrary Action of Two Helical Threads, affords great Increase of Power at the Place of most extreme Pressure. 6. Pump acting by the Difference of Two Cylinders. 7. Alphabetical System of Writing in the Dark, by Mr. John Gough. 8, 9. Two Plates, Apparatus for supplying Worm-Tubs, &c. with Water, on the Syphon Principle; By Sir A. N. Edelcrantz. 10, 11. Construction of a simple Repeater for the Hours and Quarters, By Mr. J. M. Elliot. 12. Diagram to illustrate the Investigations of the Figure of the Earth, by Professor Playfair. 13. Improved Chemical Furnace, by R. Chenevix, Esq. F. R. S. 14. Figures to illustrate an Improvement in Spectacles, by W. H. Wollaston, M. D. F. R. S. 15. New Steam Digester, in which the Heat and Pressure are rendered stationary; by Sir A. N. Edelcrantz. 16. Remarkable Strata of Flint, by Sir Henry Charles Englefield, Bart. F. R. S. 17. Figure to illustrate Dr. Herschel's Theory of the Changes in the Situation of Double Stars. 18. Mr. Hornblower's Apparatus for sweeping Chimnies. 19. Figures to explain the Changes of Double Stars, by Dr. Herschel. 20. Galvanic Apparatus to afford the Maximum of Effect in Combustion, by I. R. I. 21. Hydraulic Engine for raising Fresh Water, by the Discharge of Waste Water, by Mr. Sharples.

*Soho Square, April, 1804.*



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JANUARY, 1804.

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ARTICLE I.

*Description and Use of the Station Pointer; an Instrument for readily ascertaining the Situation of the Observer after having determined the angular Position of three known Objects. Constructed by JOSEPH HUDDART, Esq. F. R. S. and communicated by him to the Editor.*

I HAVE long been desirous of presenting my readers with a drawing and account of the instrument which forms the subject of the present paper. Its utility in protracting the situation of a vessel upon a chart from an accurate observation by the sextant of the angular position of three known objects on shore, instead of the uncertain and rough method of bearings by compass, is great and obvious; and if any other recommendation could be desired, it would be that of its repeated application in the hands of a man of science and experience. This we have the satisfaction to possess in the public labours of Mr. Huddart, whose maritime surveys are too well known and esteemed to require the suffrage of any individual at this day. The very first occasion, when I happened to have the pleasure of conversing with him, I requested the communication of this instrument, with which he with great readiness favoured me, and from which the drawing exhibited in *Plate I.* was made.

Instrument for giving a ship's place from obs. of two angles of azimuth only.

**Description.**

It is a protractor with three radii, which being set and duly placed on a chart gives the station of the observer.

It consists of a circle divided into  $360^\circ$  and filled with three radii, C B, C D and C A; the first of which is fixed at zero and the other two are moveable, and have each a nonius shewing single minutes of a degree. The centre at C is perforated in order to admit the pointer L, which is cylindrical and well fitted to the hole, where it serves to give a correspondent mark upon the chart or paper when in use. I is a magnifier carried by an arm, having its reflector K to throw light on the divisions, and its cylindrical stem H to be inserted in the hollow centre at C. As it must, while in this situation, prevent the introduction of the steel point L, the stem H is itself perforated in order to allow a smaller steel point M to be introduced for the same purpose. The radii A B and D are of brass; but they may be prolonged to an extent that would render them heavy and inconvenient if of metal, by the addition of three rulers of wood, which are attached when required by milled head screws and steady pins, as shewn by breaking the radius A, and also in the side view at G.

The arms D and B are capable of being brought very near each other, and consequently of being set to a very small angle; but A cannot, on account of the noniuses, be brought within ten degrees of D. Whenever, therefore, the left hand angle happens to be small, it becomes necessary to carry the arm A round, until by its situation on the other side of B, it comes to be employed to take the right hand angle, and consequently leaves the left to be taken between D and B.

The pointer A is provided with a clamp, by means of which, that arm can be first secured in its place, and the pointer D is then set without fear of disturbing the situation of the arm first set. It seldom happens that both angles are small in maritime surveying; so that the condition that A cannot be brought very near either of the other arms, has not been found productive of inconvenience.

In *Fig. 1. Plate III.* let A, G, and M represent three objects, of which the actual distances from each other are known and projected in a chart. Suppose an observer at H to have measured the angle  $A H M = 22^\circ.40'$ , and the angle  $M H G = 53^\circ.59'$  it is required to protract the place H by means of the station pointer.

**Solution.** Set the arms of the instrument to the respective angles, and apply them to the chart so that each point A, M, and

and G, shall be found upon the fiducial edge of its respective pointer. The center will then be found at H, which may be marked by the steel point. Description and use of the station pointer.

**Rationale of the Process.** When an observer at H sees M and A at the angular distance of  $22^{\circ}.40'$  from each other, he must himself (by Euclid III. 20.21.) be placed somewhere in a circle MAH, in which the line AM is the chord of twice that angle. And so likewise when he observes M and G under the angle  $53^{\circ}.59'$  he must be placed in another circle MGH, of which MG is the chord of twice that angle. Consequently H must be at a point common to both circles; namely where they intersect. The more direct the intersection the less will the result be affected by the errors of observation:—when the intersections are very oblique, the result will in practice become uncertain; and in the case where the two circles coincide throughout, the determination by two angles will be no better than by one; that is to say H may be any where in that circle. This last case occurs when a circle drawn through all the objects does also pass through the station itself.

In the figure already referred to H, denotes the west garret window of my house in Soho-square, and the letters A, G, and M respectively denote the steeples of the parish churches of St. Anne Soho\*, St. Giles, and St. Martin, laid down by their difference of latitude and departure from St. Paul's church, obtained from the bearings and distances in General Roy's Survey's in the Philosophical Transactions, or the *Account of Operations carried on for accomplishing a Trigonometrical Survey of England and Wales, &c. published in quarto by Captain Mudge and Mr. James Dalby. London, 1799, with 22 plates.*

The table is at pages 194, 195 of the last mentioned work. Whence and by observation with Hadley's Sextant, the data are: From St. Paul's

St. Anne's bears S.  $86^{\circ}.9'.59''$  W. 7754 feet.—Southing 518.4—Westing 7736.6 feet.

St. Martin's S.  $74^{\circ}.23'.59''$  W. 6748 feet.—Southing 1805.2.—Westing 6502.1 feet.

St. Giles S.  $94^{\circ}.36'28''$  W. 6917 feet.—Northing 55.6—Westing 6894.7 feet.

\* The steeple of St. Anne's church has been since rebuilt, and stands in the same latitude, but nine inches farther to the East than before,



Description and  
use of the station  
pointer.

Angle observed between St. Giles's and St. Martin's from  
Soho Square  $22^{\circ}. 40'\frac{1}{2}$ .

Angle observed between St. Martin's and St. Anne's from  
ditto  $53^{\circ}. 59'$ .

**Construction.** Draw the parallel of St. Paul's, and set off the points A, G, and M by the respective differences of latitude and of meridians to represent the churches of St. Anne, St. Giles and St. Martin. Join A and G, the two objects most remote in their bearings, and from the extremities A and G on the side of the line farthest from H, draw the lines Ah, Gh, making angles with A G respectively, equal to the angles observed on contrary sides of the line pointing to the middle object M. Through A, h and G describe a circle; and through M and h draw a right line, which prolonged will cut the circle in H. Join A H, G H, and the angles h A G, M H G, and A G h, M H A will be respectively equal, because standing on the same arcs h G and h A; that is to say, the objects will be seen from H under the observed angles, and consequently H will be the place of the house.

**Computation.** The numbers in the figure were had by careful construction on a scale of one inch to equal 100 feet, using a beam compass, which divides the inch into 1000 parts. But as the computation may not be unacceptable to beginners in trigonometry, I will here give the process as a conclusion to the present paper.

1. The triangle M A G is known. We have therefore the side A G and two angles given in the triangle A h G, which is thus determined.

2. In the known triangle M A G by subtracting the angle A G h from the angle A G M we gain the angle h G M which, with h G, G M, determines the triangle h G M.

3. The angle H h G is (by Euclid I. 32) equal to the sum of the interior opposite angles h G M, G M h. Therefore we have the side h G (by paragraph 1) and the two angles H and h in the triangle H h G. Consequently that triangle is known, and the distance from H, the house, to the object G may be had.

4. In the triangle M G H the angle at G is equal to the sum of the known angles M G h, h G H and the other angle at H and also the side M G are known. Whence the triangle is determined, and the distance from H to the object M may be had.

5. In

5. In the triangle  $MAH$ , the angle at  $M$  is equal to the difference of the known angles  $GMA$ ,  $GMH$ ; and the angle at  $H$ , together with the two sides  $AM$ ,  $HM$  are also known. Whence the third side  $HA$  or distance from the house to the object  $A$  may be found.

6. Lastly the bearing of any of the three sides of the triangle  $MAG$  may be had from the known difference of latitude and meridional distance of its extremities: And this bearing may with ease be then applied to shew the bearings of the lines  $HA$ ,  $HM$ ,  $HG$ .

W. N.

## II.

*Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat.*  
By JOHN DALTON.

(Concluded from Page 273, Vol. VI.)

## ESSAY III.

### ON EVAPORATION.

WHEN a liquid is exposed to the air it is gradually diffused in it. The process by which this effect is produced we call evaporation. Evaporation defined.

Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour in vacuo, which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a torricellian vacuum; hence the notion of affinity is induced. According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alcohol, ether, acids, and even metals. Water below  $212^{\circ}$  is chemically combined with the gases; above  $212^{\circ}$  it assumes a new form, and Theory which ascribes evaporation to chemical solution in air—

—is obscure and difficult.

Theory of distinct elastic vapours in the atmosphere.

and becomes a distinct elastic fluid, called *steam*: whether water first chemically combined with air, and then heated above  $212^{\circ}$ , is detached from the air or remains with it, the advocates of the theory have not determined.—This theory has always been considered as complex and attended with difficulties; so much that M. Pictet and others have rejected it, and adopted that which admits of distinct elastic vapours in the atmosphere at all temperatures, uncombined with either of the principal constituent gases; as being much more simple and easy of explication than the other; though they do not remove the grand objection to it, arising from atmospheric pressure. It has however been made to appear in these essays, I presume, that the objection to it from pressure, is itself founded upon an ungrounded hypothesis.

Leaving the theory of evaporation for the present, we shall proceed to the experiments.

The following positions have been established by others, and need therefore only to be mentioned here.

Positions. Different fluids evaporate with different rapidity. Evaporation is as the surface; it increases with the temperature; and when the air is in motion, and is drier.

1. Some fluids evaporate much more quickly than others.
2. The quantity evaporated is in direct proportion to the surface exposed, all other circumstances alike.
3. An increase of temperature in the liquid is attended with an increase of evaporation, not directly proportionable.
4. Evaporation is greater where there is a stream of air than where the air is stagnant.
5. Evaporation from water is greater the less the humidity previously existing in the atmosphere, all other circumstances the same.

The objects in view in this essay, are,

Objects of the present essay.

1. To determine the precise effect that a variation of temperature has upon the quantity evaporated.
2. To determine the ratio of evaporability of different fluids,
3. To find a rule by which the quantity and effect of previous humidity in the air may be ascertained.
4. From these and other facts to obtain a true theory of evaporation.

#### *On the Evaporation of Water at $212^{\circ}$*

Water boiled in a vessel for a definite time left by evaporation a quantity,

I took a small cylindrical vessel of tin, its diameter  $3\frac{1}{4}$  and depth  $2\frac{1}{2}$  inches; and having fixed three pieces of wire to equidistant points of the circumference, they were fastened together



ther at the top, and the extremities bent into a hook, by which the vessel might be suspended from the end of a balance, &c. This done, the vessel was nearly filled with water, which was then made to boil over a small red fire in different circumstances: it was held in the hand and removed nearer to or further from the fire, so as to be kept just at the point of ebullition. In this state the vessel and water were weighed true to a grain, and the instant of time noted by a watch; then kept as above at  $212^{\circ}$  for ten minutes or more and again weighed: and the loss of water by evaporation, per minute, was thus ascertained. The experiments were repeated several times in the same as well as in different circumstances; and the results in no instance differed materially when obtained in the same circumstances.

The least evaporation per minute was 30 grains: this was when the fire, or lamp, was in the middle of a room, the doors and windows shut, and the air calm. —which was least when the surrounding air was stillest,

The next degree was 35 grains per minute or thereabouts: this was when the evaporating vessel was over a small fire in the usual fire-place; there being a moderate draught of air, and the room close. —and gradually more,

A brisker fire, causing a stronger current of air up the chimney, gave from 35 to 40 grains per minute. —the more quickly the air was changed.

When the windows of the room were open, and a strong wind prevailed, the draught over the fire was proportionally increased, and the evaporation was from 40 to 45 grains per minute.

The extremes that have thus been noticed are 30 and 45 grains per minute: but were the experiment tried in the open air in high winds, I am inclined to believe from a comparison of the observations, that an evaporation of 50, 55 or even 60 grains per minute might be observed.

#### *On the Evaporation of Water below $212^{\circ}$ .*

I have frequently tried the evaporation at all the temperatures below  $212^{\circ}$ : it would be tedious to enter into detail of all the experiments, but shall give the results at some remarkable points. In all the high temperatures I used the vessel above mentioned, keeping a thermometer in it, by which I could secure a constant heat, or at least keep it oscillating within narrow limits. Water at temperatures below  $212^{\circ}$  lost by evaporation.—

The

The evaporation from water of  $180^{\circ}$  was from 18 to 22 grains per minute, according to circumstances; or about half of that at  $212^{\circ}$ .

Quantities  
which were,

At  $164^{\circ}$  it was about one third of the quantity at the boiling temperature; or from 10 to 16 grains per minute.

At  $152^{\circ}$  it was only one fourth of that at boiling; or from 8 to 12 grains, according to circumstances.

The temperature of  $144^{\circ}$  afford  $\frac{1}{5}$  of the effect at boiling;  $138^{\circ}$  gave  $\frac{1}{6}$ , &c.

—in every part  
of the scale pro-  
portional to the  
force of vapour  
at that temper-  
ature.

Having previously to these experiments determined the force of aqueous vapour at all the temperatures under  $212^{\circ}$ , I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the same temperature, and was agreeably surprised to find that they exactly corresponded in every part of the thermometric scale; thus the forces of vapour at  $212^{\circ}$ ,  $180^{\circ}$ ,  $164^{\circ}$ ,  $152^{\circ}$ ,  $144^{\circ}$ , and  $138^{\circ}$  are equal to 30, 15, 10,  $7\frac{1}{2}$ , 6 and 5 inches of mercury respectively, and the grains of water evaporated per minute in those temperatures were 30, 15, 10,  $7\frac{1}{2}$ , 6 and 5 also; or numbers proportional to these. Indeed it should be so from the established law of mechanics, that all effects are proportional to the causes producing them. The atmosphere, it should seem, obstructs the diffusion of vapour, which would otherwise be almost instantaneous, as in vacuo; but this obstruction is overcome in proportion to the force of the vapour. The obstruction however cannot arise from the weight of the atmosphere, as has till now been supposed; for then it would effectually prevent any vapour from arising under  $212^{\circ}$ : but it is caused by the *vis inertiae* of the particles of air; and is similar to that which a stream of water meets with in descending amongst pebbles.

In low temper-  
atures the eva-  
poration is as the  
force of vapour  
at the temper-  
ature of the  
water diminished  
by that existing  
in the atmos-  
phere.

The theory of evaporation being thus manifested from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures, regard must be had to the force of vapour actually existing in the atmosphere at the time. For instance, if water of  $59^{\circ}$  were the subject, the force of vapour of that temperature is  $\frac{1}{60}$  of the force at  $212^{\circ}$ , and one might expect the quantity of evaporation  $\frac{1}{60}$  also; but if it should happen, as it sometimes does in summer, that an aqueous atmosphere to that amount does already exist, the evaporation, instead of being  $\frac{1}{60}$  of that from boiling water, would



would be nothing at all. On the other hand, if the aqueous atmosphere were less than that, suppose half of it, corresponding to  $39^{\circ}$  of heat, then the effective evaporating force would be  $\frac{1}{2}$  of that from boiling water; in short, the evaporating force must be universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. In order to find the force of the aqueous atmosphere I usually take a tall cylindrical glass jar, dry on the outside, and fill it with cold spring water fresh from the well; if dew be immediately formed on the outside, I pour the water out, let it stand a while to increase in heat, dry the outside of the glass well with a linen cloth, and then pour the water in again; this operation is to be continued till dew ceases to be formed, and then the temperature of the water must be observed; and opposite to it in the table (p. 264, Vol. VI.) will be found the force of vapour in the atmosphere. This must be done in the open air, or at a window; because the air within is generally more humid than that without. Spring water is generally about  $50^{\circ}$ , and will mostly answer the purpose the three hottest months in the year: in other seasons an artificial cold mixture is required.—The accuracy of the result obtained this way I think scarcely needs to be insisted upon. Glass, and all other hard smooth substances I have tried, when cooled to a degree below what the surrounding aqueous vapour can support, cause it to be condensed on their surfaces into water. The degree of cold is usually from 1 to  $10^{\circ}$  below the mean heat of the 24 hours; in summer I have often observed the point as high as  $58^{\circ}$  or  $59^{\circ}$ , corresponding to half an inch of mercury in force, and once or twice have seen it at  $62^{\circ}$ : in changeable and windy weather it is liable to considerable fluctuation; but this is not the place to enlarge upon it.

For the purpose of observing the evaporation in atmospheric temperatures I got two light tin vessels, the one six inches in diameter, and half an inch deep, the other eight inches diameter and  $\frac{3}{4}$  inch deep; and made to be suspended from a balance, like the former one. When any experiment designed as a test of the theory was made, a quantity of water was put into one of these (generally the six inch one, which I preferred) the whole was weighed to a grain; then it was placed in an open window or other exposed situation for ten or fifteen minutes, and again weighed to ascertain the loss by evaporation;

Experiment to determine the force of the atmospheric vapour.

It is the same as that of water, which is at the lowest temperature, which condenses no dew on its containing vessel.

Narrative of the experiments of evaporation in atmospheric temperatures.

at

at the same time the temperature of the water was observed, the force of the aqueous atmosphere ascertained as above, and the strength of the current of air noticed. From a great variety of experiments made both in the winter and summer, and when the evaporating force was strong and weak, I have found the results, entirely conformable with the above theory. The same quantity is evaporated with the same evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the same evaporating force, a strong wind will double the effect produced in a still atmosphere. Thus, if the aqueous atmosphere be correspondent to  $40^{\circ}$  of temperature and the air be  $60^{\circ}$ , the evaporation is the same as if the aqueous atmosphere were at  $60^{\circ}$  of temperature and the air  $72^{\circ}$ ; and in a calm air the evaporation from a vessel of six inches in diameter in such circumstances would be about .9 of a grain per minute, and about 1.8 grains per minute in a very strong wind; the different intermediate quantities being regulated solely by the force of the wind.

Account of the  
table of evapo-  
ration.

The following table exhibits the ratios and quantity of water evaporated in each temperature, derived from the preceding theory, and confirmed by experiments, as far as they have been extended. The first column expresses the temperature; the second, the corresponding force of vapour taken from the preceding table; the other three columns give the number of grains of water that would be evaporated from a surface of six inches in diameter in the respective temperatures, on the supposition of there being previously no aqueous vapour in the atmosphere. These columns present the extremes and the mean of evaporation, likely to be noticed, or nearly such: for, the first is calculated upon the supposition of 35 grains loss per minute from the vessel of  $3\frac{1}{4}$  inches in diameter; the second, 45 and the third 55 grains per minute.

TABLE

Shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expressed in grains of water that would be raised per minute from a vessel of six inches in diameter, supposing there were no vapour already in the atmosphere.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

Temperature. 212°	Force of Vap. Inch. 30	Evaporating Force in Grains.		
		120	154	189
20°	.129	.52	.67	.82
21	.134	.54	.69	.85
22	.139	.56	.71	.88
23	.144	.58	.73	.91
24	.150	.60	.77	.94
25	.156	.62	.79	.97
26	.162	.65	.82	1. 02
27	.168	.67	.86	1. 05
28	.174	.70	.90	1. 10
29	.180	.72	.93	1. 13
30	.186	.74	.95	1. 17
31	.193	.77	.99	1. 21
32	.200	.80	1. 03	1. 26
33	.207	.83	1. 07	1. 30
34	.214	.86	1. 11	1. 35
35	.221	.90	1. 14	1. 39
36	.229	.92	1. 18	1. 45
37	.237	.95	1. 22	1. 49
38	.245	.98	1. 26	1. 54
39	.254	1. 02	1. 31	1. 60
40	.263	1. 05	1. 35	1. 65
41	.273	1. 09	1. 40	1. 71
42	.283	1. 13	1. 45	1. 78
43	.294	1. 18	1. 51	1. 85
44	.305	1. 22	1. 57	1. 92
45	.316	1. 26	1. 62	1. 99
46	.327	1. 31	1. 68	2. 06
47	.339	1. 36	1. 75	2. 13
48	.351	1. 40	1. 80	2. 20
49	.363	1. 45	1. 86	2. 28
50	.375	1. 50	1. 92	2. 36
51	.388	1. 55	1. 99	2. 44
52	.401	1. 60	2. 06	2. 51
53	.415	1. 66	2. 13	2. 61
54	.429	1. 71	2. 20	2. 69
55	.443	1. 77	2. 28	2. 78
56	.458	1. 83	2. 35	2. 88
57	.474	1. 90	2. 43	2. 98



Table continued.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

Temperature. 212°	Force of Vap. Inch. 30	Evaporating Force in Grains.		
		120	154	189
58°	.490	1. 96	2. 52	3. 08
59	.507	2. 03	2. 61	3. 19
60	.524	2. 10	2. 70	3. 30
61	.542	2. 17	2. 79	3. 41
62	.560	2. 24	2. 88	3. 52
63	.578	2. 31	2. 97	3. 63
64	.597	2. 39	3. 07	3. 76
65	.616	2. 46	3. 16	3. 87
66	.635	2. 54	3. 27	3. 99
67	.655	2. 62	3. 37	4. 12
68	.676	2. 70	3. 47	4. 24
69	.698	2. 79	3. 59	4. 38
70	.721	2. 88	3. 70	4. 53
71	.745	2. 98	3. 83	4. 68
72	.770	3. 08	3. 96	4. 84
73	.796	3. 18	4. 09	5. 00
74	.823	3. 29	4. 23	5. 17
75	.851	3. 40	4. 37	5. 34
76	.880	3. 52	4. 52	5. 53
77	.910	3. 65	4. 68	5. 72
78	.940	3. 76	4. 83	5. 91
79	.971	3. 88	4. 99	6. 10
80	1. 00	4. 00	5. 14	6. 29
81	1. 04	4. 16	5. 35	6. 54
82	1. 07	4. 28	5. 50	6. 73
83	1. 10	4. 40	5. 66	6. 91
84	1. 14	4. 56	5. 86	7. 17
85	1. 17	4. 68	6. 07	7. 46

Use of the table  
familiarily il-  
lustrated.

The use of this table will appear from the following problems:

## PROBLEM I.

Having given the temperature at which the aqueous atmosphere begins to be condensed into water, and the temperature of the air, to find the quantity of water that would be evaporated in a minute from a vessel of six inches diameter.

*Solution.* Subtract the grains opposite to the lower temperature from those opposite to the higher one, in the first, second or third column of grains, according to the strength of the wind, and the remainder will be the quantity evaporated in a minute under those circumstances, nearly.

*Example,*

*Example.* Let the point of condensation be  $52^{\circ}$ , the temperature of the air  $65^{\circ}$ , with a moderate breeze. Use of the table familiarly illustrated.

The number opposite  $52^{\circ}$  in the second column of grains is 2.06; and that opposite  $65^{\circ}$  is 3.16; the difference, 1.1 grain, is the evaporation per minute.

## PROBLEM II.

Having given the quantity evaporated in a minute, found by experiment, and the temperature of the air, to find the force of the aqueous atmosphere, and the point of condensation.

*Solution.* Subtract the observed evaporation from that opposite the given temperature in the table; and look above for the number nearest to the remainder, in the same column of evaporation, opposite to which will be found the force of the aqueous atmosphere, and the point at which it begins to be condensed.

*Example.* Finding the evaporation from a vessel of six inches in diameter to be 1.7 grain per minute with a brisk wind, air  $62^{\circ}$ ; what is the weight of the aqueous atmosphere, and the temperature at which it begins to be condensed into water?

The number opposite  $62^{\circ}$  in the third column of grains is 3.52, being the whole evaporating force at that temperature in a perfectly dry atmosphere; from which take 1.7 grains, the real evaporating force observed, and the remainder, 1.82, corresponds, as per table, to the force .294 inches of mercury, the weight of vapour, and to  $43^{\circ}$  of temperature.\*

*Evaporation of Spirits, Ether, &c.*

If the law of evaporation above given apply to water in every part of the scale of heat, no reasonable doubt can be entertained respecting its application to other liquids. I have notwithstanding made several experiments on others, the results of which are conformable to the same law. Some of them follow:—

Experiments which shew that the evaporation of spirits, ether, &c. follow the same law as water.

\* It may be proper to remind the reader that all the experiments on evaporation are understood to be made in the open air, or in a window with a current inward; also it may be observed the evaporation in a close room is much less and is besides irregular, being greater proportionably from a less surface, evidently from the stagnation of the air.

## 1. Spirit

Experiments which shew that the evaporation of spirits, ether, &c. follow the same law as water.

1. Spirit of wine.—Evaporated from a surface of four inches in diameter, 54 grains in 25 minutes: air  $53^{\circ}$ ; aqueous atmosphere at  $49^{\circ}$ , and beginning to rain with a moderate breeze. It would proportionally have been 121 grains from a vessel of six inches in diameter. This gives nearly five grains per minute. The same spirit boiled at or near  $180^{\circ}$ .

Now from the *data*, water of  $83^{\circ}$  is equivalent in force to spirits of  $53^{\circ}$ : and it may be seen that the evaporating force of water of  $83^{\circ}$  is nearly 5 in the first and second columns of grains of the table. It seems probable that the aqueous atmosphere does not diminish the evaporation of spirits as it does that of water.

2. Ether.—1. Put a phial containing ether, and a small tin vessel of  $1\frac{3}{4}$  inch diameter into a scale and balanced them exactly: then poured the ether into the evaporating vessel and put the phial into the scale again; took out 40 grains from the opposite scale, and waited till the equilibrium was restored: this was in 8 minutes 6 seconds. The air was  $50^{\circ}$ , and the ether at first  $50^{\circ}$ ; but it rapidly sunk, as was found by dipping a very small bulbed thermometer into it, to  $28^{\circ}$ . In a window with a moderate breeze.

2 and 3. Repeated the experiment in the same circumstances, except the evaporating vessel, which was now porcelain, and  $2\frac{1}{2}$  inches diameter. Lost 40 grains in 3 minutes. Thermometer sunk from  $50$  to  $30^{\circ}$ . The two experiments made this way did not differ above one or two grains.

These results reduced to a vessel of  $3\frac{1}{4}$  inches in diameter give

1st. Experiment, loss 17 grains per minute;

2 & 3 ————— —  $22\frac{1}{2}$  —————

The reason why the result in the first experiment was something less than in the other two, was evidently owing to the circumstance of its longer duration, by which the ether was the greater part of the time in a low temperature, and consequently evaporated less. The ether used boiled at  $102^{\circ}$ . At  $50^{\circ}$  it was therefore in the capacity of water at  $160^{\circ}$ . But water at  $160^{\circ}$ , at most loses only 17 or 18 grains per minute, and less  $20^{\circ}$  below that temperature. At first view therefore it should seem that ether evaporates quicker than the general law assigns. But it must be allowed that the temperature of the *air* has some effect upon evaporation, though it has certainly

very



very little, Now ether in the above experiments is acted upon by a current of air of an equal or higher temperature than itself; but water of  $160^{\circ}$  is usually acted upon by air  $100^{\circ}$  lower than itself, which is every moment precipitating the vapour formed, and thus obstructing its circulation. This appears to be a sufficient cause for the small difference observed.

With respect to mercury, sulphuric acid, muriate of lime, &c. there can be no doubt but they experience a real evaporation like those above; but it must be very small in proportion as their boiling points are high. And it would be difficult to make experiments upon such of these as have an affinity for aqueous vapour; because their acquisition from the aqueous atmosphere would far exceed their loss by evaporation.

Since writing the above essay, opportunities have occurred to ascertain whether the evaporation from ice is conformable to the same law as that from water. Every one, who has tried the experiment, admits the fact that ice is evaporable. I have lately made several observations on this subject, the results of which, as far as they go, support the conclusion that the general law of evaporation continues the same below the point of congelation as above it. All the experiments were made in the tin vessel above described of six inches in diameter; a quantity of water was suffered to freeze in it, so as to form a circular cake of ice; the vessel and ice were then weighed together, and exposed in the open air for a certain time, after which being again weighed, the loss was found; the force of the aqueous atmosphere was sometimes determined during the experiment by a mixture of pounded ice and salt, in the manner already described.

Evaporation of  
mercury, &c.

The evaporation  
from ice agrees  
with the prece-  
ding law.

	Gr.	H.	Gr.	Wind.	Air.	
Nov. 5. In the night lost	110 in	9 ; or, .20	per m.	N. E. brisk.	$28^{\circ}$ to $31^{\circ}$	Experimental
— at 10 A. M. —	25 in	$1\frac{1}{2}$ ; or, .33	—	N. E. mod.	$32^{\circ}$	results.
— 29. at 1 P. M. —	24 in	$1\frac{1}{2}$ ; or, .23	—	calm.	$31^{\circ}$	
— P. M. —	84 in	$9\frac{1}{2}$ ; or, .15	—	—	$30^{\circ}$	
— 30. in the night —	94 in	9 ; or, .17	—	N. E. mod.	$31^{\circ}$	
Dec. 19. P. M. —	75 in	8 ; or, .16	—	N. E. calm.	$26^{\circ}$ — $28^{\circ}$	
— In the night —	33 in	11 ; or, .05	—	calm.	$29^{\circ}$	
— 20. A. M. —	21 in	2 ; or, .175	—	W. mod	$31^{\circ}$	

Some of these being made in the night, and of long duration, neither the temperature of the air, nor the force of the aqueous atmosphere could be fairly determined: the second experiment.

experiment was made under every favourable circumstance, and the aqueous atmosphere found at  $22^{\circ}$ . By problem ii, at page 13, it would have been determined at  $21\frac{1}{2}^{\circ}$ , using the second column of grains in the table.

On the subject of evaporation it may be considered as unpardonable not to advert to De Saussure's valuable Essays on Hygrometry.

Experiments of De Saussure compared with the preceding theory.

That excellent philosopher determined, by a well conceived experiment, that dry air of the temperature of  $64^{\circ}$  or  $66^{\circ}$ , imbibed aqueous vapour so as to increase its elasticity  $\frac{1}{4}$  of the atmospheric pressure; and that a cubic foot of such air required 11 or 12 grains of water to produce the effect. By the table above at page 12 it appears the force of vapour at  $61^{\circ} = .54 = \frac{1}{34}$  of 29.5 inches nearly. It is probable this difference is occasioned in part at least by the want of perfect dryness in the air he operated upon, which caused the increase of elasticity to be less than otherwise. It was, I think, unfortunate that he attached so much importance to and confidence in his hygrometer; and that he adopted the theory of chymical solution of water in air, contrary to the facts he discovered, which seemed more reconcileable to the notion of aqueous vapour being a distinct elastic fluid. Indeed he is forced to acknowledge in the 1st. chap. of his Essay on the Theory of Evaporation, that in the ordinary temperature of the atmosphere, aqueous vapour is formed in the first instance a distant elastic fluid, and *after it has been converted into an elastic fluid*, it is dissolved by the air; "*Je crois qu'il ne la dissout que lorsque l'action du feu l'a convertie en vapeur elastique.*" Now if it can for a moment exist independently under the pressure of the atmosphere, why may it not continue to exist in that state?

and on that account his tables are inaccurate.

His table of the weight of aqueous vapour in a cubic foot of air at different degrees of the thermometer, being derived from experiments with his hygrometer, except the standard one of  $66^{\circ}$  ( $15^{\circ}$  Reaumur), is far from accurate; and the inaccuracy increases with the distance from the standard, which, as has been observed, appears to be nearly correct: in the higher temperatures he makes the water dissolved too little, and in the lower temperatures too much. He says (§ 93) that the lowest he has seen the hygrometer in the open air, is  $40^{\circ}$ ; and that it indicated a reduction of temperature in the air amounting



amounting to  $34^{\circ}.7$  ( $78^{\circ}$  of Fahr.) was necessary in order to deposit dew. This observation alone is sufficient to render his hygrometer suspected; for, few who have attended to the formation of dew will admit the probability of so large a reduction being necessary in any climate or season: I believe it rarely requires  $40^{\circ}$  reduction in temperature in any part of the world to produce the effect.

*Plate I.\** is intended to illustrate the author's conception of the constitution of the atmosphere. The different marks or characters of the particles of the gases are merely arbitrary, and intended for distinction; the simple atmospheres are given nearly on their real densities, and the particles are arranged at equal distances from each other. In the compound atmosphere the same arrangement is made of each kind of particles as in the simple; but the particles of different kinds do not arrange at regular distances from each other; because it is supposed they do not repel each other.

Engraving to  
shew the constitution of the  
atmosphere.

### III.

*Observations on the Cultivation and Growth of Oak Timber. In a Letter from the Rev. RICHARD YATES, F. A. S. Chaplain to His Majesty's Royal Hospital at Chelsea, to CHARLES TAYLOR, Esq. Secretary to the Society of Arts†.*

S I R,

TO expatiate upon the vast importance of increasing the growth of oak-timber, seems unnecessary. The national advantages resulting from this source appear to be in general well understood; and yet the cultivation and management of this most useful plant has not hitherto obtained that degree of attention which it most certainly merits.

Great importance of the culture of the oak.

Entirely to obviate, or even in some measure to remove or lessen, the obstacles that still continue to impede the planting

\* From the same Vol. of the Manchester Memoirs.—I remind the reader of the references at page 257 of our last.

† For which the silver medal was voted. Memoirs for 1802, page 80.

of oaks, would therefore be rendering an essential service to the nation. The desire of accomplishing so beneficial a purpose, has induced the judicious and public-spirited conductors of the Society of Arts to propose a premium for "ascertaining the best method of raising oaks:"—in consequence of which, this paper is submitted to their candid consideration. And as the statements here made are founded upon a sedulous and active experience of fifty years, it is presumed the *spirit* and *meaning* of the Society's proposal may have been observed, although it has not been possible (in this instance) *literally* to fulfil its terms; at least, the very intention of promoting and forwarding the views of so enlightened and highly useful a Society, may, it is hoped, be accepted as an apology for calling their attention to these observations.

Cultivation of the oak discouraged,

It forms no part of the present design to enter minutely into the various causes that continue to operate in obstructing the cultivation of oak; as there is one of peculiar magnitude, the consequences of which are highly detrimental and injurious, and which it is therefore the principal object of this paper to remove.

from a notion of its very slow growth,

An opinion is generally prevalent, that the oak is particularly slow in its growth, and requires a great number of years before it affords any advantage. This idea too often deters from planting, on account of the very great length of time it is supposed the land must be occupied before any return of valuable produce can be obtained from it, after a considerable expence may have been incurred in forming plantations.

which is a mistake.

This opinion I consider as entirely founded in error, and to have taken its rise in a great measure from the want of proper management that has hitherto commonly prevailed in the raising of oaks: and in this paper I shall endeavour strongly to state, that the oak may be rendered very rapid in its growth, and that consequently land may be employed to great advantage in its cultivation, as a very considerable and profitable produce may, in a much shorter time than is generally supposed, be derived from proper parts of an estate thus employed.

Oaks are suffered to grow (slowly) with a short stem and large head: but their *size* may be

Oak-timber in this country, for the most part, appears in trees of a considerable extent of head, but seldom more than *twenty* or *thirty* feet in stem; and this, in many instances, the growth of a century. Now, by the course of management here

here proposed, it is conceived that trees, of at least *double* doubled in half the usual time. *this magnitude*, may be obtained in about half that time.

It is not my intention to attempt a proof of this proposition by theoretical deductions, but to appeal for its confirmation to the indubitable test of fact, which, from the event of repeated trials, impresses a conviction, that experience will be found to support and establish it in the most unequivocal manner.

It would be easy to enlarge much on the various qualities of soil, the nature and process of vegetation, and the peculiar properties of the oak; but as these topics may be found amply and judiciously discussed in many other authors, who have expressly treated on these subjects, I shall decline all such speculations: and, with the hope of being more essentially useful, shall confine myself to a statement as simple and practical as possible.

The oak, in the progress of its growth, spreads numerous roots near the surface of the ground, and in an horizontal direction: these assist in supporting and preserving the tree in its position, but seem to contribute very little to its increase and magnitude. The oak appears to derive its chief nutriment and strength from a root that always descends at right angles to the horizon, and is called the tap-root. The first thing, therefore, to be observed is, that upon a judicious attention to this peculiarity, the planter's success principally depends; and the neglect of this care is the constant source of error and disappointment. In all climates, and upon all soils, to preserve this tap-root from injury, and as much as possible to assist its growth, is a general, and indeed the most essential principle in the cultivation of oak. With a due regard to this circumstance, the management of a plantation may be resolved into the three following practical directions:

*Previously* to planting the acorns, *loosen* the earth intended for their reception, by *deep trenching*. It is of the utmost importance that the perpendicular or tap root be preserved.

*Never transplant*, or in any way disturb, the saplings intended for timber. Instructions for preparing the ground and planting acorns.

Keep the plant carefully *pruned*, till arrived at a proper height.

More fully to elucidate the subject, and to prevent the possibility of misapprehension, it may be proper to give a more detailed statement.



Oaks for timber  
are never to be  
transplanted.

In determining on a spot to form a plantation of oaks for timber, it must always be recollected that the plants are to remain without removal in their first situation: the clearing and fencing may then be attended to as usual; and in the course of the winter, from September to March, the particular spots intended for the reception of acorns, may be prepared for that purpose, by digging a trench about three feet in width, and from three to six feet in depth, according to the closeness and tenacity of the soil. If grass-ground, the first spit should be placed at the bottom of the trench; and if more than one trench be necessary, they should be prepared in the same manner, preserving a distance of ten yards between each, if it be intended to employ the intermediate space in underwood, or for any other purpose.

Having made a careful selection of acorns that are perfectly sound, and in good preservation, they are to be planted about the middle of March. Draw a drill in the centre of the trench; two inches in depth, if the soil be heavy and loamy; but three inches in a light and sandy earth. In this place the acorns two inches asunder, and cover them carefully with mould. When the plants appear, they must be weeded by hand in the rows, and the earth of the trench round them cleaned with a hoe, once a month during the summer. In October inspect the rows, and thin them by pulling up every other plant: attention will of course be paid to remove the weak and crooked plants, and leave those that are tallest and straightest. On the second year, the operation of thinning must be repeated, at the same time, and in the same manner; and, should any of the remaining plants have made side-shoots stronger than the general character, they must be smoothly cut off with a sharp knife, close to the leading stem. On the third year, the thinning is again to be repeated, and the general pruning commenced, by cutting off close to the leading stem all the side-shoots of the first year; thus leaving the branches of two years to form the head of the following year. The removal of every alternate plant must be continued yearly, till the trees are about thirty feet apart, at which distance they may remain for timber. The pruning is to be continued, by removing every year, very smooth and close to the main stem, one year's growth of side branches, till the plants are arrived at a stem of forty, fifty,



or sixty feet, and they may then be permitted to run to head without further pruning.

Instructions for  
cultivating the  
oak.

The particular arrangement here recommended may be varied according to any peculiarities of situation, regard being constantly had to the general and most important principle of loosening the ground *very deep* previously to planting the acorns. By this mode of culture, oaks may be raised in almost any soil; but, where it is possible, a loam or marl is always to be chosen. Oaks thrive much the best in such earth; and, when assisted by *deep trenching* and *judicious pruning*, attain in a few years to an immense size.

Those who have been accustomed to notice the slow growth and stunted appearance of oak trees, when denied the assistance of art, and left to themselves in the common way, would observe with astonishment the vigorous and rapid increase of plants under the management now pointed out.

The plants thinned out the first three or four years, though not fit to be depended upon for timber, as transplanting generally injures very materially the future growth, may be replanted in the intermediate spaces between the rows, for the purpose of being afterwards removed; or they may be usefully placed in hedges, or other spare and unoccupied spots of ground. They should be headed down at the time of transplanting, as this operation assists the process of nature, in reproducing or remedying any injury the tap-root may have received from the removal: and, if proper attention be given to loosening the soil for their reception, and pruning them as they advance, in most instances an adequate profit will be derived from the labour bestowed upon them. After a few years, the produce of the timber-plantation will be found very advantageous. The young trees that are to be removed yearly, will always find a ready market for a variety of purposes unnecessary here to enumerate. In addition to these advantages, if by this treatment of *deep trenching* previous to planting, and *annual careful pruning* during the growth, timber can be produced in about fifty years, of equal quality, and much superior in size, to that which has been above one hundred years growing under improper management, or without the assistance of cultivation; it will doubtless be allowed that a most beneficial, if not absolutely the best possible method of "raising oaks," is here pointed out and ascertained.

This.

Instructions for  
cultivating the  
oak.

This method of cultivation may perhaps be thought to occasion so much expence in manual labour as to prevent its being generally adopted: it might perhaps be sufficient to observe, that if the work be conducted with judgment and economy, the future produce would afford ample returns for all necessary expenditure: it should also be recollected, that the previous preparation of the ground, and the subsequent pruning of the plants, are both to be performed at that season of the year when a scarcity of work will enable the planter to obtain assistance upon easier terms; with this additional advantage also, of providing employment for the labourer at those times when the general state of agricultural business renders it difficult for him to find maintenance for himself and family without charitable relief.

In 1750, at Ingestrie in Staffordshire, the seat of Lord Chetwynd, some plantations were formed and managed in a great measure according to the principles here stated, and the growth of the plants were so uncommonly rapid, and so extraordinary, that it could not but attract the notice of all concerned in the conduct of them. The attention to the subject, then excited, has been the occasion and ground of all the observations and experiments made from that time to the present, the result of which is given in this paper.

The extensive plantations of the late Lord Denbigh, at Newnham Paddox, in Warwickshire, are well known and much admired. The whole has been conducted with great judgment. About a square acre has been employed in raising oaks upon a plan nearly similar to that now proposed, and affords the best and most convincing proof of the superior utility and efficacy of such management. Had the noble Earl been now living, I should have been enabled to have laid before the Society some more detailed particulars: That, however, is now impossible; this paper, therefore, in its present state, may perhaps be thought not altogether unworthy of notice, as tending to forward the liberal designs of the Society, and contributing to the advantage of the public, the author conceiving that the best method of raising oaks is ascertained and stated in it.

Should the Society be in any degree inclined to join in this sentiment, it may perhaps induce them to make some alteration in the terms of their proposal; as, according to the statements  
made

made in this paper, and indeed from what may be seen in every part of the kingdom, in the character and appearance of oaks growing without cultivation, it seems ascertained, that “acorns set with the spade or dibble, without digging or tillage,” can never be depended on to form good timber; and even in the most favourable circumstances of this case, the growth will be exceedingly slow and precarious. The same may be said of “young plants, previously raised in nurseries, and transplanted;” for if the tap-root be cut, broken, or in any degree injured, which in transplanting it is almost impossible to avoid, that plant will seldom become a vigorous and flourishing tree. To form a course of experiments on such a plant as the oak, is not a very easy matter. To fulfil explicitly the conditions of the Society would require a great length of time, and would be attended with considerable expense, from which future candidates may in a great measure be exonerated. The raising even one acre in the manner here ascertained might be productive of great pecuniary advantage, if the facts and experience detailed in this paper are permitted to prove the inutility of the other two methods, and consequently to remove the necessity of employing so much ground upon them, at an expense they will never repay.

Instructions for  
cultivating the  
oak.

Chelsea College, Nov. 4, 1801.

#### IV.

*A First Memoir on coloured Shadows.*

*By* Cit. J. H. HASSENFRATZ.

(Concluded from Vol. VI. page 285.)

#### PART THE SECOND.

THOUGH the shadow produced by the concurrence of the solar light with that of the atmosphere, usually presents only that series of colours contained in the portion of the spectrum, between green and violet, it is not the same with the coloured shadows observed in the interior of rooms, when several lights direct or reflected concur with that of the atmosphere to enlighten the plane on which the shadow is observed.

Shadows produced by more than two lights, are more various in their tints than those before described.



observed; these are capable of shewing all the colours of the prism; in fact, we see red, orange, yellow, green, blue, indigo and violet, more or less blackened.

Number of shadows.

The number of these shadows is almost always two or three, sometimes four or five; it has even happened that we have distinguished six.

When these shadows are two in number, the colours they present are always those called *complementary* colours.

Combination of colours to imitate the solar spectrum.

The infinite variety of colours observed in the solar spectrum may be imperfectly imitated by combining different proportions of the natural colours, red, yellow and blue; and, according to the observations of Newton, an artificial white may be formed, by mixing these three colours. Those colours

Complementary colours.

are denominated *complementary*, which must be made with one or two of the three colours cited, to produce an artificial white with a given colour.

To obtain an artificial white with a red, the yellow and blue must be mixed with it. The yellow and blue make a green, green is therefore the complementary colour of red, and *vice versa*.

The complementary colour of the orange formed by yellow and red is the blue.

The complementary colour of yellow is violet.

The term does not subvert the Newtonian theory of the solar light.

As the complementary colours meet together in a great number of cases, we have thought it necessary in the outset to give them that denomination, which makes no change in the results, by which Newton proves that the solar ray is composed of an infinity of homogeneous coloured rays.

If the spectrum is divided into two parts at the point where the green commences, all the homogeneous rays of the lower part, that is to say, from the beginning of the red to the beginning of the green, are complementary to those of the upper part, that is to say, from the green to the violet; and that in the natural order of their appearance.

The denomination of complementary colours has no other object but to point out two colours which are found together very frequently; and to avoid, by this simple medium, the circumlocution which their distinction would lead to.

Complementary colours of two shadows;

Whenever two coloured shadows are seen, they are nearly always complementary to each other; that is to say, if the shade of one is red, orange, or yellow, that of the other is green, blue, or violet.

When the number of the shadows is three, one of them is of three; almost always complementary to the other two.

When there are four shadows, two of them are sometimes of four, complementary to the other two; at other times, one of the shadows is complementary to the remaining three.

When there are only two coloured shadows, one of them is always of the colour of one of the bodies which reflects the light. If the apartment, in which the shadows are seen, is commanded by a covering of slates which reflects a bluish light on the plane where the shadows fall, the two colours are blue and orange; if the covering is of tiles, the colours are nearly the same. If a meadow or trees reflect light into the apartment, the shadows are green and red. If the apartment has coloured hangings which reflect the atmospheric light or that of the sun, the shadows will participate of the colours of the lights, reflected externally and internally.

One of the shadows is of the same colour as the reflected light;  
which may be varied by another light,

When the light is reflected through glazed windows upon the plane where the shadow is seen, as the glass is rarely colourless, and generally tinged with green or violet, according as iron or manganese predominates in its composition, the shadow participates in the colour communicated to the light in its passage through the glass.

or by passing through glass,

If the surface on which the different lights are received be itself coloured, the colours of the shadow will be affected by it, and the problem will become more complicated.

or by the colour of the surface on which the shadow is received.

From the observations we have collected, it follows naturally, that the coloured shadows of the interior of an apartment are capable of yielding all the colours of the spectrum; that these colours are governed by those of the bodies which reflect the light and illuminate the surface on which the coloured shadows are observed. Thus far these observations offer nothing which could not have been foreseen by every person who had reflected a little on shadows; but that which is remarkable is, that these shadows are almost always accompanied by other shadows whose colours are complementary to the reflected colours, although no body is discovered which is capable of reflecting these sorts of colours.

Inferences.  
Other complementary shadows not produced by direct reflection.

Here then is a new shadow which seems constantly to accompany the first, whose colour has a peculiar affinity for that of the light which produces the primitive shadow, and depends on causes which we purpose to examine in a subsequent memoir.

*Part the Third.*

Shadows from  
two lights are of  
different colours.

IF in a clear moon-light night, at a time when the lamps are alight in Paris, an opaque black body be brought near a white paper, enlightened both by the moon and a lamp, two distinct coloured shadows will be perceived, one reddish, the other bluish.

If a taper, a candle, a lamp, or any other light, be brought near a white pasteboard enlightened by the moon, an opaque body placed at a small distance from the pasteboard, forms two coloured shadows; that occasioned by intercepting the light of the moon is reddish, and that produced by the other light is blue.

These two shadows may be obtained by enlightening a surface with the atmospheric light and that of a lamp; but this requires that the light of the atmosphere must enter the room where the experiment is made, through a small opening, in order that the shadow may be well defined when this light is single.

The effect was  
not varied by  
the nature of one  
combustible  
body made use  
of.

By admitting the light of the atmosphere into a darkened room through an opening of a decimetre (four inches) in diameter, and illuminating a white pasteboard by this light and that of a lamp, we have found that when the pasteboard was one or two metres (yards) from the opening, two coloured shadows were produced; that of the atmosphere is constantly red, and that of the artificial light, blue. We have employed the lights from coals, wood, tapers, candles, alcohol, and even hydrogen gas.

The greatest part of the shadows obtained from two different lights are of two tints, the one reddish, the other bluish.

The variations in the colour of shadows are independent of the intensity of the lights which illuminate the surface. We were careful in all the experiments we are going to relate, to place the light at such distances that they should enlighten equally that part of the white pasteboard on which the shadows were projected; and to this end we followed the method made use of by Bouguer, and which consists in receiving through two holes, on a piece of oiled paper, the rays from two different lights, and removing the strongest, or bringing forward the weakest, until the two lights were of equal intensity; always being careful to place the eye at an equal distance from the two enlightened points.

The



The experiments were made in a dark room, every part of which was painted black, for the purpose of destroying the colour produced by reflected light. Precautions in making the experiments.

We at first illuminated the pasteboard by the light obtained from the combustion of fish-oil in a common lamp, and by the light of hydrogen gas, produced by the dissolution of zinc in sulphuric acid weakened with water. The shadow produced by intercepting the light of the hydrogen gas, was reddish; that arising from intercepting the light of the lamp, was bluish. Shadows from hydrogen gas and oil;

At the beginning we only used a feeble light from the hydrogen gas. Being fearful that the colour of the shadows might be occasioned by the weak light, we filled a bladder with the gas, and by compressing the bladder strongly, produced a powerful bright light: the plane illuminated by this light and that of the lamp, afforded a similar result; the shadow from the hydrogen gas was reddish, that from the lamp bluish.

On enlightening the pasteboard by the light of a lamp and that of alcohol, the interception of the light of alcohol produced a reddish shadow, and that of the lamp a bluish shadow. from a lamp and alcohol;

By illuminating it with the light of the lamp and that of a candle, the shadow from the candle was black, with a light-reddish tinge; that of the lamp was black, with a light-bluish tinge. from a lamp and a candle;

The pasteboard being illuminated with the light of a lamp and that of a taper, the shadow from the taper was black slightly reddened, and that of the lamp, black a little bluish. from a lamp and a taper;

The pasteboard illuminated, at the same time, by the light of a lamp and that from the combustion of wood, the shadow thrown by the light of the wood was a violaceous blue; that of the lamp a reddish yellow. from a lamp and wood,

The experiment was tried with wood burning with difficulty, and producing little flame; with very dry wood producing much flame; with chips of wood producing a white flame, which reddened that of the lamp. When the surface was enlightened with lights of equal intensity, the coloured shadows offered little variation; that proceeding from the light of the wood was violaceous blue, that of the lamp reddish yellow. in different states.

The first experiment was repeated in glass-houses where wood is burnt. However bright the flame might be which was emitted through the working flues, the shadow produced by it on a surface also illuminated by the light of a lamp, was a violaceous blue, while that from the lamp was reddish yellow. The experiment repeated in a glass-house where wood is burnt;

On

in an iron forge; On illuminating the pasteboard by the light of a lamp and that from the combustion of culm in an iron forge, the shadow arising from the light of the culm was bluish, and that from the lamp reddish.

In a glass-house where culm is burnt; On illuminating the pasteboard by the light of a lamp and that from the culm burnt in the furnaces of a glass-house, where the brilliant flame of the working flues reddened that of a lamp, the result was similar to the preceding: the light from the culm produced a blue shadow, and that of the lamp a reddish shadow.

with a lamp and charcoal. When the pasteboard is illuminated by the light of a lamp and that from the combustion of charcoal, the shadow of the charcoal is blue, and that of the lamp red.

The pasteboard illuminated by the light of a lamp and that of charcoal urged by the large bellows in a chemical furnace, so as to produce a whitish light as well as that violaceous light sometimes observed in the furnaces of a forge and in tall furnaces, constantly afforded the same result. In these two cases the shadow from the light of the charcoal was bluish, and that from the lamp reddish. The identity of the colours of the shadows obtained in these three experiments with charcoal, notwithstanding the colour of the flame was different in each experiment, leads to an opinion that the result depends on the nature of the combustible. Nevertheless, to obtain more probability, we made further observations. We filled a chemical furnace with charcoal, and having covered it with its dome for the purpose of comparing the shadow of the direct light of the violaceous flame arising from the combustion of the charcoal, with that of the light of a lamp, we took notice of the shadows of these two lights at the moment when the flame

Probability that the colour of the shadows is produced by the nature of the combustible.

Singular change of colour in the shadows. appeared at the summit of the furnace. For a considerable time we observed that the colours of the shadows were very uncertain: sometimes the shadow of the lamp was blue, and that of the flame red; at other times the shadow of the flame was blue, and that of the lamp red. This alternate change in the colours of the shadows from blue to red, continued until the charcoal, which was in the upper part of the dome, was completely inflamed; then the colours of the two shadows became fixed, that from the flame of the charcoal became bluish, and that from the light of the lamp reddish.

Repetition of the experiment by burning the

As, from the observations of several persons to whom these results were communicated, we were induced to believe that

the

the great intensity of the light of charcoal obtained by its combustion in oxygen gas, might change or make some variation in the colour of the shadows, we ignited the extremity of a piece of charcoal, and urged its combustion by a jet of oxygen gas; the gas was contained in a bladder, which was pressed by the arm; the light obtained was extremely brilliant. A white parchment, illuminated at the same time by this light and that of a lamp, presented two coloured shadows; that from the light of the charcoal was blue, and that from the lamp red.

charcoal in oxidation  
oxygen gas,

with a similar  
result.

From the experiments we have detailed it results, that the shadow of the light of hydrogen and of alcohol is reddish, when that of the lamp is blue; that the shadow of the light of wood, of culm, of charcoal, is bluish, and that from the light of the lamp is reddish; and as hydrogen and alcohol contain less carbon than oil—and dry wood, culm, and charcoal contain less hydrogen than oil; it would seem that the blue and red colours of the shadows of the two artificial lights, bear a relation to each other corresponding with the proportions of these two combustibles; that the light produced by a substance in which hydrogen predominates, gives a reddish shadow, and that produced by a substance in which carbon predominates, affords a bluish shadow.

Summary of the  
experiments;

whence it is inferred that the colour of the shadows depends on the predominancy of hydrogen or carbon.

Although our experiments appear to lead to that conclusion, and we took great care to be certain that the red and blue shadows produced by the two lights were independent of the rapidity of the combustion and intensity of the lights; nevertheless, we dare not hazard the presenting this result as forming a general law; we prefer waiting until time and new experiments shall confirm or destroy them.

#### *Recapitulation.*

FROM the observations and experiments related in this memoir, it follows,

1st. That the shadows formed by the direct light of the sun and that of the atmosphere, vary from meadow-green to a violet-black, in a gradation through the blue, indigo, and violet; and that this variation depends on the intensity of the light (of the sun) compared to that of the atmosphere.

2d. That the shadows formed in apartments by the light of the atmosphere and reflected lights, may present all the prismatic



matic colours, more or less changed by black; and that the tints of the shadows observed are always complementary to each other.

3d. That the shadows produced on a pasteboard illuminated by artificial lights, are reddish and bluish, more or less deep; and that very probably the bluish and reddish tints of the shadows depend on the proportions of hydrogen and carbon in the combustible bodies.

The subject to  
be resumed.

We purpose, if the Institute judges this subject worthy of their attention, to make known, in other memoirs, the experiments we have made to determine the different causes which contribute to form coloured shadows in every particular circumstance.

## V.

*Account of the Preparation of the Hyacinthus non scriptus, or common Blue-Bell, as a Substitute for Gum-Arabic. In a Letter from Mr. Thomas Willis of Lime-Street, London, communicated to Charles Taylor Esq. Secretary to the Society of Arts\*.*

SIR,

Communication  
by Mr. Deyeux  
respecting gum  
from the hya-  
cintus non  
scriptus.

I HAVE observed in the Appendix of the last Monthly Review, in their Report of the Chemical Annals, No. 115, that mention is made of a letter from Mr. Deyeux, to the authors of the Annales de Chimie; in which an account is given that a gummy substance had been discovered, by Mons. Deyeux, to be contained in the bulb of the plant called hyacinthus non scriptus; and that the reviewers only say, "This article does not at present require any further notice than annunciation." As I know, Sir, your benevolent disposition in promoting and encouraging the arts, the sciences, and manufactures of this kingdom, I beg leave to offer the following observations on the above-mentioned article, which I think a subject proper to lay before your most excellent Society, and which I have no doubt may become of national utility.

\* Extracted from the Society's Memoirs from page 202: The silver medal was adjudged to this Communication.

In the year, 1794, whilst collecting plants in a wood for botanical specimens, I observed that the root of the hyacinthus non scriptus, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and on tasting it, I discovered only a very slight pungency. I collected a pound of the bulbs, and, after slicing and drying them before a fire, they yielded about four ounces of powder. I thought that by keeping the powder some time, the little acridness might go off, as it does in the arum-root powder. I tasted it about six months after, and found it perfectly insipid. I concluded it might be rendered useful for food or nourishment, but at that time pursued the matter no further.

Discovery of this  
mucilage by the  
author in 1794

In the spring of 1800, gum-arabic having been a long time very dear, and likely to continue so, I thought this mucilaginous root might answer some of its purposes, for external use. I therefore procured seven pounds and a half of the bulbs, which, when sliced and dried, produced two pounds of powder. Being soon afterwards in company with Mr. Charles Taylor, Secretary to the Society of Arts, &c. I mentioned to him that I had discovered a root which grew in great plenty in this kingdom, yielded a very strong mucilage, and which I imagined would answer the purpose of gum-arabic, in some of the manufactories. He said, if I pleased, he would send some of it down to Manchester, to be tried by the calico-printers.

Proposal in 1800  
to use it instead  
of gum-arabic.

Three or four ounces of the powder were given him, and sent down there: he was informed, upon trial, that it answered the purposes of fixing the calico-printers colours, equally as well as gum-arabic; and in the same proportion, of an ounce and a half of the powder, to four ounces of the mordant. Mr. Taylor received the samples of the printed cottons on which it had been used.

Trial at Man-  
chester success-  
ful.

On the 15th of January, 1801, I furnished Mr. Taylor with eight ounces more of the powder; but have not since heard the result.

As this root can be easily procured, and used at a less price than gum-arabic has been sold for several years past, I think it may be rendered of great utility; and the Society of Arts, &c. by patronizing it, may be the means of making it a public benefit.

The root is  
cheap.

Care

Instructions  
for collecting  
the roots and  
securing their  
growth.

Care should be taken, and advice given, that the woods should not be left destitute of the roots; and it would be advisable to offer premiums for the cultivating the roots and offsets, as they are very increasing. By such means a constant supply may be had, if the roots answer the intended purposes.

I do not presume to offer any thing respecting the mode in which the Society may think proper to divulge the discovery, and promote the use of these roots; but I imagine, that if the roots are bruised and used fresh, they would answer the purpose better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be so for two months, I wish that the discovery may be made known as soon as possible.

I have sent you specimens both of the dried roots and powder, that they may be seen at the Society's rooms, by the calico-printers. What I have done have been scorched a little in drying; but the colour would be much better, if proper care was taken in drying them.

I am, SIR,

Your most obedient Servant,

*Lime-Street,*  
*March 17, 1802.*

THOMAS WILLIS.

*To John Baker, Esq. one of the Mem-  
bers of the Society of Arts, &c.*

From the trials made before the committee with this powder, with hot and cold water, from samples of the printed cotton produced which had been printed therewith instead of gum senegal, and from experiments made in Manchester, it appears that the hyacinthus non scriptus may, in many cases, be found a useful substitute for gum-arabic.



## VI.

*A Memoir on Vinous Fermentation: by Cit. THENARD\*.*

THE vinous fermentation has hitherto received more attention than the acetous or putrid, not perhaps that it has any thing in it more remarkable, or more worthy of our consideration, but because it is in the natural order of things, to take more interest and set more value on what is of most immediate utility.

The vinous fermentation most attended to because most useful.

The date of the discovery of the vinous fermentation appears too well established to be called in question. All historians agree in saying, that the most ancient nations knew how to prepare spirituous drinks. It ascends therefore to the remotest times; and, if we pay any credit to the poets, we must carry it back to the fabulous ages. Indeed it would be surprising, if it escaped the notice of the earliest of men. An ebullition arising spontaneously in a liquid, a whole mass rising of itself, a sweet liquor becoming vinous, the change of a saccharine matter into an ardent spirit, are all extraordinary things, calculated to strike the attention, and awaken the desire of tracing them to their first causes. Accordingly there is no phenomenon more early observed, and none that has been the subject of more consideration, or has given birth to more experiments; yet, from one of those contrasts that rarely occur in the annals of science, though it has been the most studied, there is not perhaps one, with which we are less acquainted. It has been a rock, on which the endeavours of chemists in all ages have split. Becher, so celebrated for his subterranean physics, Stahl, the Nestor of the ancient chemistry, Boerhaave, whose ideas were so great, Rouelle, to whom science is indebted for part of the progress it has made during the last half century, and Macquer, that master in the art of writing, all failed in their attempts to penetrate this mystery of nature. Lavoisier, who was capable of surmounting the greatest obstacles, is the only person, who, enlightening the whole sphere of chemistry by his genius, travelled this obscure path without wandering out of his road. His inquiries into fermentation will ever remain a model of vegetable analysis. In this, as in every thing else

Known to the most ancient nations.

Its phenomena excite attention,

and have led to many experiments;

hitherto little known.

All chemists have failed to explain it,

except Lavoisier who alone made some progress in it.

His investigation is a model of vegetable analysis.

\* *Annals de Chimie*, June 1803. p. 294.

But he left it imperfect.

In fermentation sugar is converted into alcohol and carbonic acid, by means of an intermediate substance. But what is this? and how does it act? Imagined to be extractive matter, mucilage, tartar, which last is not found in all fermenting liquors,

or a mixture of all these.

he did, he observed that strictness of deduction and accuracy of operation, which are his characteristics, and which may be considered as the source of the splendid discoveries, that will for ever illustrate his name. Still, notwithstanding the excellence of what he did, he was far from leaving nothing more to be done. Though he shed great light on this part of science, the obscurity with which it was enveloped was so great, that it is still seen only through a mist. This is a truth that did not escape himself; he was well aware that he had only laid open the path to the goal, which no doubt he would have reached, no doubt he would have completed the career he had begun with such success, had not death, jealous of his fortune and glory, robbed science of his labours.

All we know of fermentation in fact is confined to this, that the saccharine matter is converted into alcohol and carbonic acid by means of an intermediate substance. But what is the nature of this substance? and how does it act on the sugar? These two grand questions form the subject of the present memoir; questions that have been often attempted, without ever having been solved. Some have thought, that the fermenting principle resided in the extractive matter. Others would have it to be in the mucilage, because this more frequently accompanies the extractive matter. Some, again deceived by the presence of tartar in wine, have imagined they found in this the true ferment; but if, instead of confining themselves to the fermentation of the must of grapes, they had turned their attention to that of other juices, in which analysis cannot discover the existence of this salt, they certainly would not have fallen into this error. Others, lastly, inconsiderately adopting all these opinions, have asserted, that a mixture of these different matters preside as it were over fermentation, effecting the decomposition of the sugar, and its conversion into alcohol\*.

Of these hypothesis some are evidently false; others are seducing, and acquire from specious reasoning some degree of probability. But before we admit them, we must consult ex-

\* Mr. Thenard here gives a long note, to prove he had not seen the prize essays written in 1787 and 1788 by Fabroni, from whom some persons at Paris accused him of having borrowed, and to show, that Fabroni and he differ essentially in this theory; but it is here omitted as irrelevant. T.

perience;

perience; we ought, leaving them all out of the question, to deduce from observation the theory, which we are too apt to form beforehand. If the genius of Stahl, instead of giving birth to phlogiston, a being that never existed but in the brilliant imagination of that extraordinary man, had inter-rogated nature by means of experiment more than he did, perhaps it would not have gone astray; perhaps Stahl would have discovered the truth, and deprived France of the glory of having produced the author of the modern theory of chemistry. Such is the course I have pursued. Before I formed or adopted any system, I observed facts, and deduced from them consequences, which, it appeared to me, must guide us to the view of what passes in liquors under fermentation. But in a subject of such nicety nothing is more easy than to deceive ourselves; and it is particularly for the purpose of correcting my notions, if they be not just, that I submit to the class the result of my inquiries\*.

Stahl misled by hypothesis and neglect of experiment.

Facts observed by the author.

My first observations were made on the juice of gooseberries, which I had strong reasons to prefer to any other; its fermentation proceeding with most celerity, so that it is consequently best calculated to throw light on the causes that produce it. All my researches were directed at first to discover the matter, that serves as a ferment. It would be making a great step, and almost resolving the problem, or at least discovering a number of truths not yet known, to determine the nature of this matter, and to ascertain whether it be always one and the same, or whether there be several that possess this property. This important question had struck me long ago: I had even meditated upon it occasionally, and promised myself to attempt its solution, when the Institute proposed it as a subject of a prize. This was an additional strong motive to my pursuing it. I was far from being disposed to admit several fermentative principles; every thing led me to believe, that there was but one, and that it was none of those hitherto suspected, since in fact neither extractive matter, nor mucilage, nor tartar, &c. acts upon sugar. But this required positive demonstration; and though I have yet no absolute proofs of it, as it is by no means demonstrated, that there are several, and one is ob-served on all occasions, this opinion seems to me at present to deserve the preference.

He first examined gooseberry juice, as fermenting most quickly;

in order to discover what serves as the ferment,

and whether it be one peculiar substance, or several.

He supposed it to be one and the same;

and this, though not demonstrated, most probable.

\* This paper was read to the National Institute.



Gooseberry juice contains a slightly glutinous matter, which, separated by filtration,

occasioned a mixture of sugar and water to ferment,

and become vinous.

But as it scarcely effects this on six times its weight; it may be presumed to contain but a small portion of the fermentative principle. This could not be obtained apart; not apparently altered by having produced fermentation; but it ceased to yield ammonia.

The germ of fermentation is therefore of an animal nature,

not separable by reagents.

The author therefore had recourse to fermentation.

Through a linen cloth of close texture I pressed out the juice of a killogramme of gooseberries. It was turbid, and held in suspension a slightly glutinous matter, which I separated by the filter, and washed in a large quantity of water. As nothing is to be neglected in experimental science, and the most trifling fact frequently leads to important consequences, I subjected this matter to a regular examination. I first mixed it with sugar and water, to see whether it would cause them to ferment; and I soon perceived many bubbles of an elastic fluid to be extricated, which I found to be carbonic acid. The effervescence continued a week, and at the end of this period the liquor was a pleasant drink, but slightly saccharine; it contained a great deal of alcohol, and might easily have been mistaken for a wine not yet completely made. It may be supposed I redoubled my zeal and attention in the examination of a substance, that offered me what I sought. It was natural first to inquire, whether the whole of it were adapted to decompose sugar; but a sixth part of its weight being scarcely able to effect this decomposition, I concluded, that it contained the fermentative principle only in small quantity. This I attempted every method in vain to separate, and obtain apart: nothing therefore remained for me, but to compare it before and after it had served to produce fermentation. It was not apparently altered by this process; being still insipid, insoluble both in water and in alcohol, and affecting neither infusion of litmus nor syrup of violets: but on distillation it no longer afforded any trace of volatile alkali. This result, at which I was not surprised, and which a second experiment confirmed, was nevertheless a ray of light, that confirmed me in the course I should pursue. It showed me, that the germ of fermentation was of an animal nature, it agreed with the ideas I had before conceived, and gave to my suspicions an appearance of reality.

I now examined the juice of gooseberries with great care, to discover this animal matter, which I considered already as the true ferment. As it was insoluble by itself, it must be combined with some substance, that held it in solution. All the reagents I employed failing to answer the purpose, I had recourse to fermentation itself, and observed the phenomena produced by it under all circumstances. I made my experiments on nearly a litre of filtered and perfectly clear juice.

The

The apparatus was placed in a stove, where the thermometer stood at  $20^{\circ}$ : it was not long before a fermentation was evident; a large quantity of carbonic acid was presently evolved; much froth was formed; the liquor lost its transparency, and it even became so turbid, that a sediment was thrown down, which was more evident as the fermentation approached its end. This sediment was of a yellowish white colour, glutinous, void of taste, grew brown on drying in the open air, and became slightly acid. Thrown on red-hot coals it burnt in the same manner as animal substances: distilled in a small retort it afforded a considerable quantity of carbonate of ammonia even crystallized. It made sugar ferment with extreme promptitude. In short it was a substance perfectly analogous to the yeast of beer.

I was eager to try whether this phenomenon were general, as it ought to be according to my mode of reasoning: and in fact experience soon taught me, that it was common to all juice in a state of fermentation. The must of grapes, the juice of cherries, pears, peaches, and apples, and the decoction of barley and of wheat, afford yeast in their fermentation. The grape juice yielded more than the others, though less than that of gooseberries: accordingly it did not ferment so readily as the latter. The juices of cherries and peaches deposited nearly the same quantity: those of pears and apples afforded very little, which is the reason why their fermentation is so slow. I could have wished to have had a greater number of fruits at my disposal, that I might have varied my experiments more: they were sufficient, however, to prove, that, where alcohol is formed, a sediment of yeast is commonly formed likewise. If they who have any doubt on the subject remaining will maturely consider the two following experiments, I believe they will find themselves convinced. I knew that honey diluted with water would gradually be converted into a liquor containing spirits. Cullen informs us, that the saccharine urine of a diabetic patient undergoes in time the same change\*. Accordingly I set both of these to ferment, and the sediment of yeast was formed in each.

Filtered gooseberry juice, in a heat of  $20^{\circ}$ , soon fermented; evolved much carbonic acid, was covered with froth, became turbid, and deposited a sediment of a yellowish white, glutinous, insipid, grew brown on drying, and slightly acid. On red-hot coals burnt like an animal substance, and afforded carbonate of ammonia when distilled. It caused sugar to ferment; and is perfectly analogous to yeast. This substance common to all fermenting juices. Grape juice yields most next to that of gooseberries. The juices of pears and apples afford little, whence they ferment slowly. Yeast commonly formed where alcohol is produced; even from honey and water, and diabetic urine.

\* It was Dr. Dobson of Liverpool, who first found, that diabetic urine changed first into a vinous liquor, and afterward into an acetous, before it became putrid. T.



So that in every spirituous fermentation an animal matter is deposited similar to yeast.

Is this generated in the process of fermentation?

No direct proof, that nature employs it exclusively as a ferment.

If it be a product of fermentation, it probably comes from some particular substance, little changed.

Yeast an immediate principle of vegetables.

and if there be any other ferment, it must be similar to it, and composed of azote, oxygen, carbone, and hidrogen.

It may be laid down, therefore, as demonstrated, that in every spirituous fermentation an animal matter is deposited, similar in all respects to that arising from wort, possessing absolutely the same properties, and in particular that of decomposing sugar, and converting it into carbonic acid and spirit of wine. This gives rise to a new question, that naturally presents itself, and ought next to occupy our attention. Is the yeast generated in the process of fermentation, or rather was it already formed, and did serve as a ferment?

I must confess we have yet no experiments, which directly prove, that nature employs this substance exclusively to effect the conversion of sugar into alcohol and carbonic acid. For why should it be deposited when the fermentation has taken place? It may be said indeed, that the sugar holds it in solution, that it can dissolve more than is necessary for its decomposition, and that then the excess is precipitated. But this theory is feebly supported by experiment. Is this a sufficient reason, however, to reject it altogether? Have we not several instances of compounds, that require much time for their formation? and this perhaps is what occurs in the juices of fruits, where the ferment and the sugar are long in contact with each other. What is certain, or what at least appears probable, is, that, if yeast be a product of fermentation, as all liquors that ferment deposit it, no doubt it owes its origin to one and the same soluble substance, from which probably it differs little, and which produces it by its reaction on the sugar.

Whichever of these two opinions obtains the preference, after more numerous trials, as I have no doubt, that yeast is an immediate principle of vegetables, and in consequence acts an important part in the phenomena both of art and of nature; as I am likewise persuaded, that, if any other substance capable of exciting fermentation exist, it is in the highest degree analogous to yeast; that it differs from it very little; that it is composed like it of azote, oxygen, carbone, and hidrogen; and lastly that it has no doubt the same mode of acting on sugar: I shall proceed to exhibit with the greatest care the properties of this matter, which I shall henceforward term ferment, and in particular consider well its action on the saccharine principle, in order to establish the theory of fermentation. This theory, even supposing yeast not to exist already formed, but to be produced in fermenting juices, will still be of utility, and capable of various applications, as will appear below.

I shall



I shall not recapitulate the physical qualities of ferment, as I have mentioned them several times already; but I shall confine myself to its chemical properties, which alone are of essential import. It has no taste. It neither reddens infusion of litmus, nor changes syrup of violets green. The putrid fermentation, which in time it undergoes, is in every respect similar to that of animal substances. By desiccation it loses three fourths of its weight, and this loss consists entirely of water. Thus dried it is still capable of exciting fermentation; it is by no means decomposed, and may be preserved in this state without alteration for an indefinite time. We may avail ourselves of this property, to convey it to places at a distance from any brewery, or with which it is so difficult to keep up an intercourse, that fresh yeast could not be sent to them, particularly in summer, without becoming putrid. Distilled in a small retort, and urging the fire so far as this would bear, eight parts of ferment left a residuum of 2.83 of coal; and I obtained 1.61 of water, 1.31 of oil, and 1.46 of muriate of ammonia on adding muriatic acid. Finally, I collected 0.33 of gas, containing a fifth of its bulk of carbonic acid, and which, when this was separated from it by potash, burnt like carbonated hydrogen, and required 1.5 of oxygen for its combustion. From this experiment we see, that ferment contains in particular a great quantity of carbon.

Chemical properties of ferment.

May be preserved any length of time when dried.

Analysis.

Water at the temperature of 12° or 15° does not dissolve  $\frac{1}{400}$  of ferment: indeed it dissolves so little, that after standing upon it several hours, and being well filtered, it has scarcely any action upon sugar. Boiling water occasions it to undergo a decomposition, which I shall examine in another memoir.

Insoluble in water.

Boiling water decomposes it.

Nitric acid, even diluted with water, at eighteen degrees, decomposes it also: it converts it into grease; and there is evolved from it at first azot mingled with carbonic acid, and afterward nitrous gas at the same time.

Nitric acid converts it into fatty matter; and azot with carbonic acid, and nitrous gas are given out. With potash it forms soap, and ammonia is extricated.

Potash acts upon this substance in the same manner as upon animal matters, and the phenomena in both cases are perfectly the same: with each it forms a saponaceous substance, and a great quantity of ammonia, that is volatilized.

Its action upon sugar.

But of all the properties of ferment, no one is so remarkable, and at the same time so useful, consequently no one so much deserves to be studied, as its action upon sugar: this is interesting to men in every class of society, from the mechanic to the

the philosopher; to both by its products, and to the latter because it may be a fertile source of reflection and of new truths. It is much to be regretted, that Lavoisier did not pursue the investigation as he intended, and examine it with that care, which is conspicuous in all his labours. Who was more capable of giving birth to a theory of fermentation, than the author of the modern theory of chemistry? No doubt he was prevented from doing this by a concurrence of circumstances: and this theory, important as it is to science, has hitherto remained vague and hypothetical. Knowing the fermentative principle, it could not avoid naturally making a part of my researches: if I have not rendered it as clear as I hoped, at least the veil with which it was covered is removed, and it rests on reasoning confirmed by experiment.

Of the precipitate one part with five of sugar fermented readily.

Products of carbonic acid and spirit obtained,

equal to 171.5 of alcohol.

The residuum 12 grammes, nauseous and, slightly acid.

Lavoisier says the acid is the acetous.

Residue of the ferment.

To obtain the solution of this problem, I added together different quantities of ferment and sugar; I observed in every case what became of both; and I confirmed by farther observations what the preceding had suggested. Sixty grammes (927 grains or nearly one ounce of ferment) not dried, and three hundred grammes (4630 grains) of sugar entered into fermentation readily, the temperature being  $15^{\circ}$  (centigrade =  $59^{\circ}$  Farenheit.) In four or five days all the saccharine matter had disappeared; 51.5 litres (3041 cubic inches) of carbonic acid had been evolved; the liquor being filtered, and distilled to two thirds, gave on a second rectification 863 grammes of spirit at  $13^{\circ}$ . The apparatus was so contrived, that nothing was lost: the receivers were cooled with common salt and ice. I found by synthesis, that this quantity of spirit was equivalent to 171.5 grammes of alcohol at  $39^{\circ}$ . The residues left after distilling the spirit were poured into dishes and evaporated to dryness: from the residuum of the second distillation nothing was obtained, but that of the first yielded about 12 grammes of a nauseous substance, slightly acid, and feebly attracting the moisture of the air. I wished to discover the nature of this acid, but there was too little to ascertain it. Lavoisier says, it is the acetous. Lastly, of the sixty grammes of ferment, there remained forty grammes of a substance, which I believed to be more animalized than the ferment itself. I was much surprised to find, that I obtained from it by distillation much less ammonia. Hence I suspected, that by mixing it afresh with sugar, fermentation would again take place, and thus

thus all the azot would disappear. What I had foreseen occurred: at the end of seven days, having filtered the liquor, I obtained as a residuum thirty grammes of a substance, which on distillation gave no trace of volatile alkali. I was persuaded, that the azot was carried off with the carbonic acid gas. To convince myself of this, I collected near 41 litres of the carbonic acid in an inverted vessel filled with solution of caustic potash. The whole was absorbed, which leaves no doubt of its purity.

Produced fermentation afresh, and thus lost all its ammonia.

This was not carried off with the carbonic acid.

What then becomes of the azot? it ought to be found either in the residuum of the ferment, or in the residuum obtained by evaporating the liquor left after distillation, or in the alcohol: but the residuum constitutes only half the ferment employed; the quantity of matter left by 300 grammes of sugar and sixty of ferment amounts only to twelve grammes; and neither of these yields any ammonia on distillation, while ferment affords a great deal. If these observations be just, if I have accurately noticed all the phenomena, if nothing have misled me, we cannot avoid concluding, that the azot must exist in the alcohol. Yet I have sought in vain to discover its presence in this fluid, in ether, and in the acetous acid: on passing these through tubes heated red-hot in the fire, and burning the gasses in Volta's eudiometer by means of the electric spark, we obtain such small quantities, that they are by no means sufficient to decide the question: 24 or 25 parts of gas yield at most one of residuum.

What became of the azot?

It must exist in the alcohol, though not discoverable there.

I have made several other experiments however, which hitherto tend to show, that azot may exist in such a manner as not to be discovered on distillation; consequently that it may be one of the constituent principles of vegetables, though in general when distilled they afford no ammonia. But as I have not repeated these experiments, and they are of such importance, that we cannot be too reserved in announcing them, I purpose to revise and vary them, I will endeavour to appreciate all the circumstances, and if I obtain convincing proofs, I will not delay communicating them to the class.

Azot may exist so as not to be detected by distillation, and therefore may be a constituent principle of vegetables.

However this may turn out, these results afford us sufficient light, to see what passes in the act of fermentation. In this respect I cannot agree in opinion with Lavoisier. I do not believe as he does, that all the carbonic acid formed proceeds from the sugar. How, on such a supposition, can we conceive the

All the carbonic acid formed does not proceed from the sugar.



Ferment produces fermentation by taking from the sugar a portion of its oxygen.

Ferment has a strong attraction for oxygen, as is proved by its decomposing air.

What becomes of the principles of ferment.

Leaves a peculiar substance.

Yeast is seldom pure, containing more or less of this peculiar substance, with which it forms the chief part of wine-lees.

the ferment to act upon it? I think, that the first portions of acid are owing to a combination of the carbon, of the ferment, and the oxygen of the sugar, and that the ferment gives rise to fermentation by abstracting from the sugar a portion of this principle. To render this idea more clear, I suppose a particle of sugar to be formed of eight parts of oxygen, four of carbon, and one of hydrogen, which is not very remote from the truth, according to the experiments of Lavoisier: one of these eight parts of oxygen will unite with a fourth of a part of the carbon of ferment, and then, the equilibrium between the principles of the sugar being disturbed, they will combine in a different manner, so as to form carbonic acid and alcohol. The ferment has in fact a strong attraction for oxygen; as is proved by its decomposing air with the greatest facility; when acetous and carbonic acids are produced, and the azot is disengaged. If pure air be employed instead of common air, the reaction is still more speedy. I have introduced 15 grammes of ferment into a vessel filled with a litre of pure air; I opened it over quicksilver; a fifth of its bulk was absorbed; the ferment was grown sour, all the oxygen gas had evidently disappeared, and was converted into carbonic acid: the temperature was  $15^{\circ}$ .

From what has been said we see what becomes of the carbon of the ferment, and we shall learn what may become of its other principles, if we recollect the quantity of products resulting from a given quantity of matter subjected to fermentation, and comparing the nature of the one with the other.

From sixty grammes of ferment, and three hundred grammes of sugar we obtained, carbonic acid 95 grammes; pure alcohol 171.5; extractive matter, slightly acid, and containing no azot, 12; residuum of the ferment 40. These 40 grammes still contained 25 of ferment, so that 35 only had been employed for the decomposition of the sugar; and these 35

were reduced to 15 of a white substance\* insoluble in water, incapable

\* The yeast deposited by a fermenting juice is seldom pure: in general it contains more or less of this peculiar white substance, which is sometimes so abundant, that the sediment consists of it almost entirely. It is the mixture of these two substances, that constitutes in great part the lees of different wines. All these effects depend on the relative proportions of the ferment and the sugar; if there be little sugar and much ferment, the yeast will be pure; if

on

incapable of acting upon sugar, yielding no ammonia on distillation, and leaving a coal, that burns with scarcely any residuum; in short, exhibiting characters that distinguish it from all other substances, and induce me to consider it as a peculiar matter.

It appears then, that ferment takes oxygen from sugar, not only by means of part of its carbon, but also by means of part of its hydrogen; for the quantity of carbon given out by ferment is too small, to be the sole germe of fermentation. The azot disappears, and enters perhaps into the composition of the alcohol; the other principles of the ferment form acetous acid, and a peculiar insoluble white matter, which is precipitated. The acetous acid remains in solution in the liquor left after distillation, with an extractive matter, proceeding no doubt from the sugar, and foreign to it.

The hydrogen of the ferment acts as well as its oxygen in producing fermentation.

It is not probable, that the elements of the sugar, in their reaction upon each other when the equilibrium between them is disturbed, form water: there is very little hydrogen in sugar, and a great deal in alcohol; and besides, on adding together the quantity of carbonic acid, of alcohol, of extractive matter, and of residuum, we have a deficiency of one eleventh only of the matter, by which they were produced. This loss must be attributed to the water the sugar contains, and is by no means owing to alcohol's being carried off by the carbonic acid. Of this I have been convinced myself by receiving more than thirty litres of this gas in caustic potash; and by distillation and rectification I obtained only a few grammes of fluid, which had so little taste of spirit, that it was not to be distinguished.\*

The elements of the sugar do not form water.

A loss of one eleventh owing to water contained in the sugar. No alcohol carried off by the carbonic acid.

This

on the contrary there be much sugar and little ferment, the sediment formed will contain but little yeast, and perhaps even none; and then it will consist entirely of the white substance here mentioned, while the supernatant wine will or may be of a saccharine quality, Thus should yeast even be a product of fermentation, it would act an important part in this phenomenon; and if it do not give rise to it in the first place, which is not certain even in this case, at least it continues to give rise to it. This was the reason why I said before, that in all cases nature afforded us numerous applications of the theory I laid down.

This depends on the proportion of the ferment to the sugar.

\* Cit. Seguin has laid before the Institute a very different theory of fermentation. He thinks, that in this process water is decomposed, Seguin's theory of fermentation.

This theory agrees with the facts, though not yet complete.

What remains to be done.

This theory, appears to me so much the more probable, because it perfectly accords with facts; and this truth becomes particularly striking when we compare them. I am far, however, from considering it as complete. Time no doubt will bestow on it the perfection it wants; and I hope myself soon to add to its evidence. I know not whether I shall be able to discover what becomes of the azot of the ferment: but I shall ascertain without difficulty, whether the residual matter obtained, which I consider as a peculiar substance, be a product of fermentation, as I believe; whether the sugar contribute to its formation, which is possible; or whether it be ready formed, and merely precipitated, which is contrary to all probability. The experiments, that remove all my doubts on this head, I shall relate in a second memoir, in which I shall not only elucidate such points in this as may appear equivocal, or at best resting on too slight foundations, but I shall also exhibit all the particulars that result from them. Here on the contrary I have avoided them as much as possible, and endeavoured to consider the phenomenon only in a general way.

## VII.

*Conjectures respecting the Formation of the Ice in the Cavern of Grace-Dieu. By C. L. CADET, of the College of Pharmacy\*.*

A cavern 146 feet underground, its entrance 60 feet broad and 80 high, its breadth 135 feet.

ABOUT six or seven leagues from Besançon, near the village of Beaume, and half a league from the abbey of Grace-Dieu, there is a natural grotto, 146 feet below the surface of the earth. It is supposed, that its oxygen unites with the carbon of the ferment, and produces carbonic acid, while its hydrogen combines with the sugar, and converts it into alcohol. For this theory to be admissible, we should obtain more alcohol than there was sugar; but the fact is, little more than half its weight is produced, and besides, supposing all the carbon of the decomposed ferment to be converted into carbonic acid, at most not above a sixth part of the quantity actually produced would be formed, as we may easily convince ourselves from calculations already established. It may likewise be objected to this theory, that sugar contains a great deal of oxygen, and alcohol very little.

\* *Annales de Chimie*, No. 134, Feb. 1803, p. 160.

the



the plain. The entrance of it is 60 feet wide, and about 80 high. Within its greatest breadth is 135 feet. The stone that forms the rock is calcareous carbonate, in part lamellated.—

This grotto is distinguished from all others by a very singular phenomenon: in summer ice is constantly formed in it in large quantities, and this ice diminishes at the approach of winter.

Ice is formed in summer, and diminishes in winter.

As this natural ice-house has been accurately described by Mr. de Croismare in the *Encyclopédie*, and several other descriptions agreeing with his are to be found in the *Memoirs of the Academy of Sciences* for 1712, those of the *Savans Etrangers* for 1743, the works of Mr. le Cat and Ravier, and Madame Ganthier's *Tour of a French Lady in Switzerland*, I shall confine myself to a few observations, that have escaped the notice of many of the learned who have visited it, and combine to explain the formation of the ice in this cavern.

Described by various writers.

I visited it about the end of September, 1791, and then found but little ice. My guide, who was accustomed to conduct travellers to it, informed me, that a month before the ice was three times the bulk of what I saw. The person who sells refreshments at Chammars, a public garden at Befançon, told me the same. He said, that when the winter had proved mild, so that they were not able to fill the ice-houses in the city, he sent in search of ice to the cavern of Grace-Dieu, and that he chose for this the warmest days, *because then there was most ice* in the cavern. Messrs. de Croismare, le Cat, and Ravier, in their accounts, equally attest, that in summer the ice is more abundant. The inhabitants whom I met with on the mountain, and of whom I enquired concerning this fact, all told the same thing. Consequently, though the variation of the ice is questioned by Mr. Dunod, quoted in the work of Madame Ganthier, I readily admit the existence of this phenomenon, particularly as I think I can explain it.

In September, 1791, it had but little ice.

A month before it had thrice the quantity. Ice there in mild winters.

By several authorities the ice is most plentiful in summer.

The rock that forms the roof of this grotto is lower than all the neighbouring mountains, and even than the surface of the plains. Its temperature therefore ought to be mild, like that of all subterraneous caverns, except at the moment when ice is accumulated there in abundance. Ten waggons would have been sufficient to carry away all I saw, and the air of the grotto did not feel colder to me than the open air. The authors I have quoted say, that the cold in it is below 0. I had no thermometer, but I can affirm that this assertion is at least questionable.

The rock that forms the roof low.

The air in it does not feel cold.

Description of  
the cavern.

Clear water  
bursts through  
the rocks and  
falls into a basin  
of ice.

The grotto is  
covered by  
bushy trees.

The ground is  
cooled by the  
evaporation in  
summer,

which does not  
take place in  
winter.

The trees over  
the entrance  
being cut down,  
less ice was  
formed.

The entrance  
was partly closed  
up by a wall,

by which the  
ice was dimi-  
nished.

A just idea of the situation of the cavern is sufficient to shew in what manner the water percolates the strata of rock, and falls perpetually from the roof. The cavern is surrounded by thickly wooded mountains, and from one of the nearest rocks rushes a torrent, that forms three cascades, and turns several mills. But what is remarkable is, that the water filtering into the cavern does not freeze as it falls: it is received into a basin of two or three feet diameter formed in the ice, and remains fluid on a level with the sides of the basin. I drank some of this water, which did not seem to me very cold. It was perfectly clear and good.

I observed, that the grotto is not only surrounded by wood on all sides, but that it is covered by a number of the most beautiful beech, and bushy chestnut trees. These deny all access to the rays of the sun, and always preserve a cool damp air round the cavern. This shade however would not be sufficient to produce an icy air, but the leaves of these trees are so many organs of perspiration, that pour into the atmosphere the moisture which exhales from their vessels, and that has been pumped up by their roots. This evaporating water cannot pass from the state of a fluid to that of a gas, without absorbing a large quantity of caloric: the ground therefore is continually cooled by the evaporation the trees produce. In winter the warmth of the sun no longer draws up moisture; the leaves, which botanists consider as real lungs, have discontinued their organic action; and the same evaporation no longer exists. The temperature of the cavern then changes, and approaches that of subterranean places and grottos in general. Several facts serve to confirm this theory, and to prove, that the cold produced in the cavern of Grace-Dieu is entirely owing to the speedy evaporation of the surrounding moisture.

Mr. Ravier, secretary to the bishop of Bellay, says, that in 1724, the entrance of the grotto was overshadowed by large bushy trees, that these were cut down, and that the quantity of ice afterwards formed has been less.

He also reports, that Mr. de Vanolles, intendant of the province, desirous of preserving this natural curiosity, closed up the grotto by a wall twenty feet high, with a small door, the key of which was entrusted to the bailiffs of the village, with a charge, that, no one should be suffered to enter for the purpose of carrying away the ice. But this precaution produced

an unexpected effect: the ice perceptibly diminished, and they were obliged to pull down the wall, which was in some measure an obstacle to the evaporation. I saw the remains of this wall, and the inhabitants of Grace Dieu confirmed to me the fact mentioned by Mr. Ravier.

The formation of the ice in this curious grotto, therefore, may be explained, I believe, according to the theory of the jars that serve to cool the water they contain in hot countries, by permitting part of the fluid to transude through their porous sides.

This ice stated to be formed in the same way as water in porous jars is cooled in hot countries by the evaporation from the outside.

As no natural philosopher has yet attempted to explain this interesting phenomenon, I have flattered myself that I might be indulged in a conjecture, founded on physical principles fully established by experience.

### VIII.

*Observations on the Structure of the Tongue: illustrated by Cases in which a Portion of that Organ has been removed by Ligature.* By EVERARD HOME, Esq. F. R. S.\*

(Concluded from Vol. VI. Page 379.)

ENCOURAGED by the result of this case, I was led to perform a similar operation upon a person at a more advanced period of life.

Another case treated in the same manner,

Margaret Dalton, 40 years of age, was admitted into St. George's hospital, on the 25th of December, 1801, on account of a tumour, the size of a pea, situated on the right side of the tongue, near its edge. The history of the case was as follows. A small pimple appeared, and gradually increased, without pain; the only inconvenience was, that it affected her speech, and, when bruised by the teeth, bled freely.

The operation was performed on the 11th of January, 1802, in exactly the same manner as has been already described. It produced a considerable degree of salivation, which was extremely troublesome, (much more so than the pain the ligatures produced,) and continued till the slough came away. The ligature nearest the root of the tongue separated on the 6th day; the other on the 7th; and, in three days after the separation of the second ligature, the wound was completely skinned over.

with complete success.



## A third case.

A third case of this kind came under my observation, in which there was a small tumour in the substance of the tongue, about the size of a pea, which gave me the idea of its being of that kind which might terminate in cancer. The patient was a gentleman of about 41 years of age. Upon examining the tumour, I told him of my alarm respecting its nature; and at the same time added, that I was very ready to remove it, should it be the opinion of other practitioners that such a step was adviseable; and my experience in two former cases led me to believe it might be done with safety. I therefore advised him to consult other medical practitioners of reputation, and acquaint me with their opinion. Mr. Cline was consulted, and his opinion coincided with mine; which made the patient decide upon having the tumour removed.

The operation,  
and history of  
its progress and  
successful termination.

The operation was performed on the 28th of December, 1802. The needle pierced the tongue an inch beyond the tip, a little to the right of the middle line of the tongue; and the space between the two ligatures, when they were tied at the circumference of the tongue, was fully an inch. The tongue was thick; and the mass included by the ligatures was such as to make it difficult to compress it. The operation gave considerable pain, of a numbing kind. Immediately after the operation, the part included became dark coloured, particularly towards the middle line of the tongue. A salivation took place. The next day, the pain and salivation were great, and the patient could not swallow; but, on the day following, he could take broth, negus, and other fluids.

On the 6th day from the operation, the slough became loose; and the least motion of the tongue gave great pain. Upon examining the slough, there was a small spot which looked red, and was surrounded by a dark surface; this was towards the right side. Upon further examination it appeared, that the ligature to the right had not completely deadened the part at the centre, in which the artery had its course. This accounted for the red spot, as well as for the pain the patient suffered; and led me, on the seventh day, to disengage the ligature on the left, (which was almost completely separated,) by means of a pair of scissors, and pass another ligature through the groove to the opposite side, and tie it over the part not completely deadened. This gave great pain for a few hours, which was relieved by the use of tincture of opium.

opium. On the 8th day, the patient had less pain than on any preceding day, and less salivation; and, on the 9th, the whole slough came away. On the 13th, the tongue had so much recovered itself, that there did not appear any loss of substance whatever, only a fissure of half an inch in depth, in the anterior part of it; and, as that now seemed to be exactly in the centre, there was not the smallest deformity.

The preceding cases, in the view which it is intended to take in the present Paper, are to be considered as so many experiments, by which the structure of the tongue is in some respects ascertained: they enable us to draw the following conclusions.

General results of the preceding cases or experiments.

The internal structure of the tongue is less irritable than almost any other organized part of the body; therefore, the peculiar substance which is interposed between the fasciculi of its muscular fibres, is not in any respect connected with the nerves which pass through its substance to the organ of taste, but is merely a soft medium, to admit of great facility of action in its different parts.

Internal structure of the tongue has little irritability.

The nerves of the tongue appear to be more readily compressed, and deprived of their power of communicating sensation, than nerves in general; and any injury done to them is not productive of diseased action in the trunk of the injured nerve.

its nerves easily deadened.

If we compare the effects of compression upon a portion of the tongue, with those of a similar compression upon the hæmorrhoidal veins when they form piles, or those of the testicle in cases of varicose veins of the spermatic chord; which not only produce very violent local inflammation, but also a considerable degree of symptomatic fever, it is impossible not to be surprised that the results should be so very different; since we are led to believe, upon a general principle, that parts are sensible in proportion to their vascularity, and that all the organs of sense, when inflamed, are more exquisitely so than any other parts of the body.

Comparatively little inflammation and pain follow compression of this organ.

The tongue appears to have a power of throwing off its sloughs in a shorter time than any other part. Eight or nine days is the ordinary time of a slough separating from the common parts: in the boy's tongue, it was only five.

It sloughs off very early.

Having stated the information we derive from these cases, respecting the structure, sensibility, and irritability of the



tongue, it now remains to mention the advantage to be derived from them in a professional view; and, although this is not directly in the line of the pursuits of this learned Society, yet, so strongly is it connected with humanity, that it cannot be said to be foreign to them, or undeserving their attention.

Great benefit of this knowledge in cases of cancer.

The information derived from these cases, enables us to attempt with safety, the removal of any part of the tongue which may have taken on a disposition to become cancerous. As this disease in the tongue always begins in a very small portion of that organ, it is, in the early stage, more within the reach of removal than when in any other part of the body; and, as the glands of the tongue are independent of each other, the cancerous disposition by which one of them is attacked, does not so readily communicate itself to the others; and the part may be removed, with a greater degree of security against a future recurrence of the disease, than in other cases where this malady attacks a portion of a large gland, the whole of which may be under the influence of the poison, long before there is any appearance of its being diseased.

## IX.

*Account of Three Mechanical Structures not commonly noticed or known, in which the Effect is produced by the Difference between the Actions of Two simple Instruments of the same Description; namely, 1. a compound Barrel and Winch; 2. a compound Screw, and 3. a compound Pump.* W. N.

Properties of the wheel and axle.

IN the simple mechanical engine, called the wheel and axle, or barrel and winch, the advantage of the power over the weight is known to be as the radius of the wheel or winch is to that of the axle or barrel. If it were, therefore, proposed to increase that advantage, the handle must either be made longer or the barrel more slender; neither of which can practically be done beyond certain limits in the usual form; because the handle might be too long for management, or the barrel too slight to support its burthen. The celebrated George Eckhardt is, I believe, the inventor of the construction, *Plate III. Fig. 2.* in which the part A of the barrel is larger than the part

A compound barrel or roller, by which the purchase may be greatly augmented.



part B, and the rope which passes under the pulley C and sustains the weight D is wound upon each in contrary directions. Whenever, therefore, the handle is turned, so as to gather the rope upon the larger cylinder, it will be given off by the smaller: and, for every turn of the larger or its correspondent portion of rope wound up, there will be given off a portion of rope, answering to the circumference of the smaller. Consequently, the quantity of unwound rope will be less, after such a turn, by a quantity equal to the difference between the circumferences of the two cylinders; and the weight D will be elevated through half that space. Whence

As the circumference described by the winch (or its radius) *Its power*

Is to half the difference between the circumferences of the cylinders (or half the difference of their radii)

So is the power

To the weight (in equilibrio.)

It is evident that the peculiarity of this engine is that the half *Peculiarity of this engine.* difference of the radii may be made as small as we please without weakening the cylinder or requiring any quick curvature of the rope; neither of which requisites could be had in the simple cylinder. Its practical disadvantage appears to be that a large quantity of rope will be used to produce a moderate elevation of the weight.

The first account I find of the double-screw *Plate IV. Double screw engine.* *One screw presses down while another draws up, and the effect is produced by the difference of their actions.* Fig. 1. is given by Mr. Wm. Hunter in the Philosophical Transactions for 1781. In the sketch before us, A B C represents the screw of a press; the upper part of the screw A has a coarser thread than the lower B and a concave screw or nut, C, receives the latter, while the former passes through a proper helical cavity in the frame of the press. In the present state of the apparatus, the nut C is pinned fast to its screw by a cross bolt; and consequently, when the whole screw is made to turn by the arms at the top, the nut C is carried round, and the effect is precisely the same as if the press were governed by the screw A alone. But when the nut C is disengaged by drawing the cross bolt, and likewise prevented from revolving by pinning it to the moveable platform, the effect becomes more compounded: that is to say, while the whole screw is carried downwards by one revolution, through one thread of A, the nut C is drawn upwards through one thread of C. And

the consequence is the same as if a single screw had been used, having the distance between thread and thread equal to the difference between the measures of one thread in each.

Examples of  
slow actions  
with strong  
coarse screws:

For example. Suppose it were required to operate by strong pressure with a screw of ten threads in the inch; it is evident that the metallic protuberance or helix could not be quite so thick as one twentieth of an inch, and could not therefore withstand any considerable force. But by the present construction we may make the thread as strong as we please. Thus if the slightest be one quarter of an inch, the finest screw must have two turns in the inch, and the coarsest two turns in one inch and one tenth; or in other words, it would pass over the same space in ten turns as the other screw would in eleven.

It is evident that by the construction in the figure, the most powerful pressure need not be used till the subject under compression has already been urged by the simple screw.

Observation on  
new or unusual  
constructions.

I may here remark concerning every one of the contrivances in this paper, that I have never considered it as a requisite in offering a machine or instrument to the attention of my readers that it should be universally better than the means already in use. If it be new or unheeded, and ingenious, or capable of adding to the stock of resources which intelligent constructors find so various in their degrees of eligibility under different circumstances, it will, I am confident, be considered with interest, and accepted with welcome.

Considerations  
respecting the  
use of a double  
screw in mea-  
suring.

When we consider the extreme precision of a simple screw, if commonly well made, it may seem scarcely necessary to use this contrivance in the measurement of small quantities. The finest screws usually to be met with do not exceed 100 threads in the inch, and I do not know of any having been made finer than of 200 threads in that space. Let us consider by what threads of usual or even coarse number we could have a difference per turn of above one thousandth of an inch, and we shall find that the difference between a thread of 32 and one of 33 in the inch is the 1056 part of an inch. So likewise the difference between the 60th and 61st of an inch is the 3660th part, &c. I have a level trier on this construction which gives the four hundredth of an inch for each turn of its compound screw.

Pump with a  
double plunger  
by which the

Fig. II. Plate 4. is a pump described to me by Mr. Bramah, and I think his own invention. Between the collar of leather,



A, there passes a solid cylinder, of which the lower part B (diminished in diameter) passes out through another collar of leathers D; the internal space or pump barrel situate between those two collars being closed on all sides, except where two pipes communicate, one, of which, E, has a valve opening inwards, and the other D, has a valve opening outwards. Now when the solid cylinder or plunger is drawn up, the larger portion passes for the most part out of the cavity of the barrel; but instead of being replaced by an equal mass of water entering through the pipe E as usual in the jack head pump, it is mostly replaced by the introduction of the solid part B, and the water which enters will be equal in its solid contents to the difference only between the solidities of the two cylindrical parts of the same length. Or the effect will be the same as if the barrel were of much smaller dimensions. In this way with the strength of construction and ample fittings of a large engine the power of a small one may be obtained, which in some instances of forcing may be desirable.

It scarcely need be mentioned that the same effect may be produced by two solid pistons working in distinct barrels of different bore or stroke, and communicating with each other.

## X.

*Scoteography, or the Art of Writing in the Dark. In a Letter from Mr. JOHN GOUGH.*

To Mr. NICHOLSON.

SIR,

PERHAPS you have remarked that my communications to your Journal are not all in the same hand: the truth is, I am unable to write; and my ignorance of the art arises from the loss of sight in early infancy. The knowledge of the preceding circumstance cannot prove of the least moment to you; it is only mentioned by way of introduction to the present letter, which may afford an useful hint to persons labouring under the same disadvantage.

The perusal of Mr. Berard's method\* of teaching the blind

\* Phil. Jour. Nov. 1802, Vol. III. page 190.



to write was perhaps the first occurrence of my life which led me to think seriously of committing my thoughts to paper with my own hand.

Berard's method of writing by the touch only is not adapted to the early blind.

This gentleman's plan however presented many difficulties which it is unnecessary to enumerate; in reality it promises to be of much advantage to those who have learned to manage a pen before they lost the use of their eyes: but as this has not been my lot, I resolved to conquer the hardships of fortune, if possible, by inventing an alphabet consisting of characters which a mechanical apparatus might form with ease and certainty. After sketching an outline of a method in my own mind, I imparted my unfinished project to an ingenious friend, being induced to ask his assistance by a consciousness of my own inexperience in the art of writing. This gentleman completed the business in a short time, by inventing a set of distinct characters of easy execution, and contriving the means of imprinting them upon paper.

Requisites of the art.

The origin of the invention being now stated, the process may be described in the following manner; including the preparation of the paper, the alphabet, the law of combining the letters, the apparatus, and the proper kind of ink.

Practice.

The paper is ruled into square compartments.

The paper is prepared by dividing it into several square compartments, which will appear in the annexed figure, page 57. The operation might be performed by means of a ruler, but the page may be stamped with greater certainty by the writing-frame; which instrument will be described in the sequel.

The alphabet is formed by placing a dot or a stroke in a different part of the square for each letter.

The alphabet is formed by means of two marks only; these are a dot, and a straight stroke; for each compartment may be considered as the skeleton of any character the writer wishes to make; and it is completed by placing the requisite mark in its proper place. The letters might be described separately, but a moment's inspection of the alphabet given in *Fig. 1st* explains the structure of it better than words can do. The scheme moreover shews, that there are thirteen places where the marks may be fixed, so as to form all the letters: to these may be added the three characters denoting the end of a word, a capital, and the numerals 1, 2, 3, &c.

Method of combining the letters or of writing.

Since placing a dot or a stroke in a square makes a letter, the same compartment may form the basis of several characters in certain circumstances; provided the law of combining them be first established.

My method consists in supposing the thirteen places of each compartment to be numbered, as is done in the annexed figure (*Fig. 2d.*) Now if, in spelling a word, the place of the former of two adjacent letters be of a less denomination than that of the latter, they may both be put in the same square; in like manner if the second do not exceed the third in value, this last may also be inserted with the other two; and the same rule will obtain with the fourth, &c. Letters of the same denomination, as o, r, &c. are to be placed in separate squares; and every capital must have a compartment to itself. This way of writing observes no proportion betwixt the length of a word and the space occupied by it: the monosyllable (and) fills three squares; which is the number taken up by the trisyllable (inspection) See *Fig. 1st.*

The elementary processes of the art being now explained, we come in the next place to the description of the instruments and their uses.

The writing-table is an oblong board, having two parallel brass plates passing from bottom to top, and containing a space between them, just sufficient to receive a sheet of paper: they are an inch in breadth, and  $\frac{1}{8}$  of an inch in thickness, being fixed with their flat sides to the table. A row of short pins is placed along the middle of either plate: each pin in one row is opposite to one in the other, and the distance of two adjacent pins in either plate, is exactly equal to the breadth of the writing frame. This instrument is an oblong piece of brass, a quarter of an inch thick: it has a bevil at each end, which parts pass over the side plates of the table, and thereby suffer the intermediate parts of the frame to rest upon the paper. The annexed figure (1st) shews the structure of the perforated part and the size of the squares.

The writing table.

The writing frame.

The process of stamping the paper consists in covering the underside of this plate with printer's ink; a small hole being made in either bevil for the reception of two opposite pins in the side plates; the operation commences by placing the frame on the highest pair, it is afterwards successively removed to each inferior pair until the whole page is stamped and prepared for the pen.

Operation of preparing the paper.

This instrument is a square brass rod, four inches long: The pen. each side of the square being nearly half as long, as a side of



of a compartment in the frame. One end of this pen is cut away parallel to one of its sides, so as to form a plate three eighths of an inch long and a line in thickness: the end of the plate is also filed down so as to leave at one corner a point, which is parallel to the axis of the rod: this point is used in making dots. The other end of the rod is cut diagonally parallel to its axis, thereby forming a prism, the end of which is an isosceles triangle. The extremity of this prism is also cut away, so as to leave a plate lying in the same plane with that described above: the end of this plane has a small bevil externally, which gives it the edge of a chissel; and it is used in making strokes. The writing frame always rests on that part of the page, which you are going to write upon; and the sides of the compartments together with their angles guide the pen to the proper places: the method of using which will easily occur to those who are acquainted with the alphabet and apparatus.

Ink.

They should however be informed that *printer's ink*, is fittest for the purpose: it is spread upon a cushion of soft leather, which must be touched by the pen, as oft as it is applied to the paper.

JOHN GOUGH.

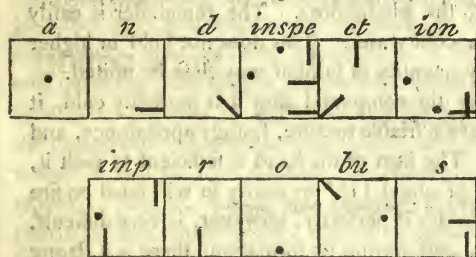
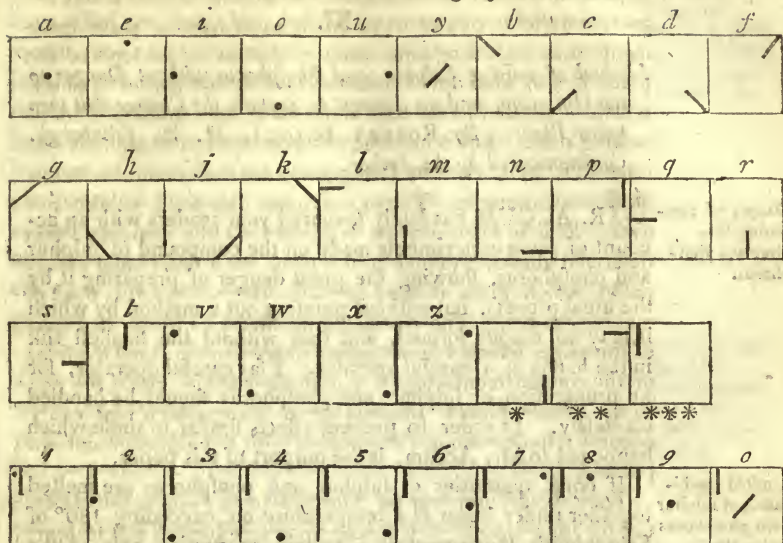
*Middleham, Dec. 15, 1803.*

(See alphabet, next page.)

P. S. A paper of mine was read Nov. 4th before the Manchester Society, in which I endeavour to demonstrate Mr. Dalton's doctrine of mixed gases to be repugnant to the mechanical philosophy. It also proves experimentally that water augments the bulk of air of a given temperature without the intervention of a stopple of mercury; from which the non-existence of an aqueous atmosphere is inferred upon known principles.

ALPHABET.



ALPHABET. *Fig. 1st.**Fig. 2.*

1	12	11 10
2		
3	13	9
4 5	6	8 7

*Denominations of the Letters.*

1	2	3	4	5	6	7	8	9	10	11	12	13
b	l	i	c	m	o	d	n	u	f	p	e	a
g	q	h		r	j		s	k				y
v		w			x			z				

\* The square distinguished by one star underneath contains the mark to denote the end of a word.

\*\* The square so distinguished contains the mark denoting a capital letter.

\*\*\* The square so distinguished contains the mark signifying a numeral character or number.

## XI.

*Method of uniting Sulphur and Phosphorus without Danger to the Operator, and an Attempt to explain the Change that then takes Place. By ROBERT BRIGGS, M. D. Edinburgh. Communicated by the Author.*

Danger of combining sulphur and phosphorus.

MR. ACCUM has lately favoured your readers with an account of some experiments made on the compound of sulphur and phosphorus, showing the great danger of preparing it by the usual process, but without pointing out a method by which it may be readily formed, and that without the smallest risk in the hands of a careful operator. I say careful operator, for no preparation of sulphur and phosphorus should be handled carelessly. In order to prevent effects similar to those which happened to Mr. Accum, is the purport of this paper.

Limited combination of sulphur with phosphorus under  $180^{\circ}$ .

If equal quantities of sulphur and phosphorus are melted together under water at a temperature not exceeding  $180^{\circ}$  of Fahrenheit's thermometer, a certain proportion only of the sulphur unites with the phosphorus. The remainder is easily washed away with cold water. This does not hold in higher temperatures: any quantity of sulphur may then be united.

Qualities of this compound.

Upon examining the compound after it is perfectly cold, it will be found to have a friable texture, spongy appearance, and a sulphur colour. The heat of the hand is sufficient to melt it, and a temperature of about  $112^{\circ}$  or nearly so will set it on fire if perfectly dry. To dry it perfectly, however, is very difficult. When just formed, and during its formation, there is a strong smell of sulphurated hydrogen, and a considerable quantity of that gas makes its escape. After the heat of the compound under water, is raised to  $200^{\circ}$  or  $210^{\circ}$ , the water is decomposed very rapidly; white fumes, which before filled the empty space of the flask, now issue out of its mouth in abundance, and if the vessel is shaken by the hand or by allowing the water to boil, a sudden inflammation and explosion is the consequence. The compound of sulphur and phosphorus begins to decompose the water immediately after the union of the two commences, but, towards the boiling point of water, that decomposition is very rapid.

If the heat be raised nearly to boiling, the water is rapidly decomposed, white fumes escape, and it explodes.

If



If you take the vessel of the fire just when the white fumes have filled the flask, but, before they begin to issue from its mouth, and place it in a dark room without shaking it in the least, the whole space above the water which is filled with the dense white fumes, has a beautiful appearance, like an aurora borealis. At this time very frequently, though not always, bubbles of phosphorated hydrogen gas, or rather sulphur phosphorated hydrogen gas, if I may be allowed the term, escape and take fire on coming in contact with the external air, forming a beautiful white ring of smoke rising to the ceiling of the room,—just such as happens from the decomposition of water by means of phosphuret of lime or potash.

The white fumes are beautifully luminous in the dark, and fiery rings sometimes appear.

Now what is the change produced during this formation? That the water is decomposed, is abundantly evident. But while the hydrogen of the water escapes, what becomes of its oxygen? If the oxygen were also allowed to escape, we should feel its presence, from explosion after explosion taking place, immediately after it left the water along with the hydrogen, united with as much phosphorus as is sufficient to fire the two.

In this process, though the water is decomposed, the oxygen is not set free.

The firing, however, only happens after the sulphur phosphorated hydrogen reaches the external air, or when the decomposition goes on very rapidly from a high temperature, and faster than the oxygen can be disposed of. From all these facts taken together, I believe it must be pretty obvious, that the compound which we form of sulphur and phosphorus under water, is not of pure sulphur and phosphorus, but of their oxides. That is, during the junction of sulphur and phosphorus by heat under water, part of the water is decomposed, the hydrogen escapes after engrossing a small portion of sulphur and phosphorus, while the oxygen unites with the sulphur or phosphorus, or both, converting them into an oxide of sulphur and phosphorus. This will be found farther confirmed from considering the nature of this compound. It is very inflammable, much more so than a compound of pure sulphur and phosphorus formed by another method which I shall describe by and by; but which may be converted into an oxide by a very simple process, which I shall also point out; then it becomes one of the most inflammable bodies with which I am acquainted. Farther the compound of oxide of sulphur and phosphorus, does not decompose cold water so far as I have been able to perceive, though the contrary is said by Mr. Accum; but it will decompose the water if heated.

It is employed in oxidizing the bases.

which oxide is more inflammable than the simple compound.

Phosphorus and sulphur do not decompose water unless heated.



The compound first mentioned is of the oxide ; and the phosphorated and sulphurated hydrogens contain oxygen.

Best method of forming it.

The mixture heated in a phial covered with a perforated bladder.

Explanation of Mr. Accum's accident.

Upon the whole then, I imagine, that, this compound when formed under water, is nothing else than a mixture of the oxide of sulphur and the oxide of phosphorus, and that phosphorated hydrogen gas, is not phosphorus dissolved in hydrogen gas, but a solution of the oxide of phosphorus in that fluid, and the same may be said of sulphurated hydrogen. It is not sulphur, but an oxide of sulphur that is dissolved.

I shall now proceed to mention what I consider as by far the best method of forming a junction of sulphur and phosphorus, and of afterwards converting them into oxides without risk or danger.

When I first began to examine the nature of the oxides of sulphur and phosphorus, the decomposition of water, and the escape of its hydrogen only, at a low temperature, I imagined that I could effect the union of the two much better without the water.

I took a phial glass and filled one third of it with the usual proportions of sulphur and phosphorus, I tied a piece of strong bladder over the mouth of the glass, and made a small hole in it with a pin. I expected that after the phosphorus began to melt, probably it would fire with the small quantity of oxygen of the atmospheric air in the phial, before the sulphur and phosphorus had joined. I therefore used the precaution of a small hole in the bladder to allow the escape of the rarefied azotic gas. It did fire, and was followed by a trifling explosion.

The heat was by this suddenly augmented, the union of the sulphur and phosphorus formed immediately, and nothing more particular happened. The heat, however, is raised so suddenly that if the phial is large it is very apt to break. I would not therefore recommend this to be tried by a young chemist without great caution.

Before going farther, I would now attempt to explain the accident that happened to Mr. Gardenas related by Mr. Accum. The compound of oxide of sulphur and phosphorus which he was melting with the phosphorus, had either not been thoroughly freed from water, which it is very difficult to do, and hence the explosion is easily explained on the principles which I have already related ; or, allowing that there was no water, the phosphorus had been fired by the oxygen of the atmospheric air in the upper part of the phial, at the very moment of shaking

shaking the glass. This of course would raise a quantity of the melted phosphorus, and at the same time oblige him to remove his finger from the mouth of the phial. The highly rarified nitrogen would rush out with violence, carrying part of the melted phosphorus along with it, an explosion and its effects would be the consequence.

A pretty loud report may be produced by heating any portion of air in a phial glass covered by the finger, on suddenly removing the finger.

This explanation, however, is rendered abundantly evident from what follows. Fill a phial glass, or what is still better, a thin glass tube shut hermetically at one end with sulphur and phosphorus. Cork it firmly, and plunge the tube into hot water, the heat of which is to be gradually augmented till it boils. The sulphur and phosphorus will melt and unite peaceably. Shake them after taking the tube from the boiling water, that the mixture may be complete, and no accident will happen.

Experiment in proof. Phosphorus and sulphur gradually heated, in a small closed vessel, unite without danger.

It is hardly necessary to caution against plunging the tube into boiling water at first. A contrary plan allows any small quantity of oxygen that may be present in the very small portion of air that may remain between the particles of the sulphur and phosphorus to be taken up by the phosphorus before melting. If a compound of sulphur and phosphorus be prepared as above directed, having only a small proportion of sulphur, it has a yellowish white colour, is solid when cold, and has a crystallized appearance. This substance is more inflammable than phosphorus, but not by any means so much so as a dry oxide of sulphur and phosphorus. This compound, however, may be easily converted into an oxide by the following simple process. Set fire to the mixture, still in the tube, by taking out the cork, admitting a little air, and plunging a hot wire into it; allow it to burn only five or six seconds. It will by this be converted into the oxide of sulphur and phosphorus, be found to resemble in appearance very much a neutral salt efflorescing, sublime into the neck of the glass, and be so very inflammable that the instant you bring it into the air it catches fire, though the thermometer be at the freezing point.

Not so if the heat be sudden.

This compound is less inflammable than the first mentioned oxide.

into which it may be converted by combustion.

*Edinburgh, December 15, 1803.*



## XII.

*On the best Method of ascertaining the Dip at Sea. In a Letter from Mr. EZEKIEL WALKER.*

To Mr. NICHOLSON,

SIR,

The dip of the horizon rendered very uncertain by refraction.

The back observation will not correctly shew the dip.

It is best found by taking the sun's altitude and its opposite supplement by two persons ;

or by one person with two instruments.

IT is well known that refraction varies so much near the horizon as to render the depression or dip very uncertain, whence it seems desirable, in the present improved state of navigation, to find the dip by observation rather than trust to the table. An instrument so constructed as to measure an angle of  $180^\circ$  would be very convenient for this purpose, and the back observation in Hadley's quadrant has been proposed ; but the back observation is attended with so many inconveniences to the observer, and so little to be depended upon, as to render it nearly useless. The instrument which I have proposed for taking any angle less than  $180^\circ$  seems well adapted to this purpose \*. The distance of the two opposite horizons at sea is  $= 180^\circ +$  twice the dip ; but the motion of the ship renders this method very troublesome and uncertain ; for whatever uncertainty attends taking an altitude of a celestial object, that uncertainty is doubled in taking the distance of the two opposite horizons. To avoid this inconvenience, let the meridian altitude of the sun's upper or lower limb be taken with a sextant by one observer, and at the same time, let another observer, with an instrument so constructed as to measure an angle of  $180^\circ$  take the distance of the same limb of the sun, from the opposite horizon ; the sum of these two angles— $180^\circ$  is  $=$  twice the dip. And as two observations of the sun's altitude are thus taken independently of each other, the latitude will be obtained with greater precision than by a single one.

The observations may also be taken with two instruments by one person.

Let the observer begin some time before noon, and take the altitude of the sun's limb. 2. With the other instrument take the distance of the same limb from the opposite point of the

\* See Philosophical Journal, vol. iv. p. 218. and vol. vi. p. 219.  
horizon.



horizon, then take the first instrument, and correct the observation; and so continue to use the instruments alternately till the sun shall arrive at his greatest altitude.

If the sun's altitude were taken, when off the meridian, by two observers, in the manner above mentioned, in order to find the hour from noon, it might greatly add to the accuracy of that important problem. This method is recommended in finding the time.

## REMARKS.

From the above strictures it appears, that navigation might receive many improvements from an instrument that would take in the largest angle, and admit of being accurately adjusted. The latitude and time of the day might then be determined with greater precision than at present, and the limits of the method of finding the longitude by the lunar theory would be enlarged.

EZEKIEL WALKER.

Lynn, December 22, 1803.

*(To be continued in a future communication.)*

## ERRATUM.

Vol. VI. p. 219. near the bottom, for  $120^{\circ}$ , read  $180^{\circ}$ .

## XIII.

*New Process for fabricating Alum artificially and without the Assistance of Evaporation. By Cit. CURAUDAU, Corresponding Member of the Society of Apothecaries of Paris\*.*

A REPORT was made to the National Institute in Fructidor of the year 9, by Citizens Guyton and Vauquelin, on a new process of Citizen Curaudau, for the artificial fabrication of alum. As this report is not generally known, we hasten to communicate it to our readers.

Citizen Curaudau proposes, in the fabrication of alum, to employ one hundred parts of clay, and five of muriate of soda, diluted with a quantity of water sufficient to give a pasty consistence to the mixture. It is then made into loaves, with

Process employed by Cit. Curaudau to make alum artificially. 100 clay and 5 common salt kneaded with which water; the loaves

\* From the Annales de Chimie, Floreal, An. XI. No. 137.

are ignited,  
cooled, pounded,  
and one fourth  
of sulphuric acid  
water is added  
by degrees till 10  
times the acid.

which a reverberatory furnace is filled, and wherein a brisk fire is kept up for two hours, or until the inside of the furnace is obscurely red. The calcination being finished, the clay is reduced to powder and put into a sound cask; one fourth of its weight of sulphuric acid is then poured over it, at several intervals, agitating it strongly upon each addition. As soon as the disengaged vapours of muriatic acid are dissipated, a quantity of water equal to that of the acid is added, and it is stirred as before. This produces a combination between the acid, the earth, and the water, so rapid, that the mixture heats, swells and emits abundance of vapours. Lastly, when the heat has abated a little, water is continually added until the quantity amounts to about eight or ten times that of the acid.

To the clear  
decanted liquor  
potash is added.

When that part of the earth which is useless in the formation of the alum is deposited, and the liquor become clear, it is drawn off into vessels or boilers of lead. A quantity of water equal to that of the liquor drawn off, is then poured over the residuum, and afterwards added to the first: lastly, a solution of potash is mixed with the lye, in which the alkali amounts to one fourth of the weight of the acid employed, and it is agitated. If the preference be given to employ sulphate of potash, the quantity used must be double that of the alkali.

The alum crys-  
talizes by cool-  
ing.

After a certain time, the liquor by cooling, affords crystals of alum, the quantity of which, when the crystallization is completed, amounts to three times the weight of the acid made use of. This alum is refined by dissolving it in the smallest possible quantity of boiling water; and it is then as pure as the best alum of commerce.

The washings  
employed in a  
subsequent  
operation.

As the residue still retains some saline parts, Citizen Curau-dau recommends washing it a third time with a quantity of water sufficient to extract all the salt, and to use this instead of simple water in a second operation; by which means, there will not be any loss.

The mother-  
waters useful  
in making  
prussian blue.

Thus, without the assistance of heat, the greatest part of the alum formed in the operation is obtained, which is highly beneficial. The author recommends the use of the mother-waters, which still contain alum with sulphate of iron very much oxidized, in the fabrication of prussian blue, to which purpose they are well adapted.



He considers the fabrication of alum as particularly advantageous to the manufacturers of prussian blue, because they may calcine their clay at the same time with their animal matters, without any increase of expence. They will have no need in that case to add potash; and the presence of iron, instead of being injurious, will, on the contrary, be very useful. If they are desirous of fabricating alum for sale, they may use the solution of sulphate of potash arising from the washing of their prussian blue, and which is usually lost, instead of water, to dissolve the combination of alumine and sulphuric acid. The residuums or refuse of the distillers of aquafortis are equally applicable to the same purposes: they contain the alumine and potash requisite to the composition of alum, and it will be sufficient to sprinkle this substance, reduced to powder, with sulphuric acid, and to add the necessary quantity of water to the mixture, proceeding as has been directed above. The mother-waters of these alums are also useful in the fabrication of prussian blue.

This process very beneficial to the makers of prussian blue.

Refuse of the distillation of aquafortis may be used.

We may also observe on this occasion that the refuse of the distillers of aquafortis contains more alkali than is required for the saturation of the sulphate of alumine formed by the clay, and that to obtain the greatest possible quantity of this substance, it is necessary to add an eighth by weight of this earth calcined, according to the process of Citizen Curaudau; and that in using about sixty parts of acid to a hundred of this earth, at least one hundred and eighty parts of very fine alum are obtained.

It contains an excess of alkali.

Citizen Curaudau affirms, that he has for a long time prepared alum by these different processes with an advantage of more than 25 per. cent. over that of commerce, and that, notwithstanding the price to which it has now fallen, he still derives from 10 to 12 per. cent. profit. Lastly, the manufacturers of prussian blue, to whom the potash would be no expence, would at this time gain from 17 to 18 per. cent. by making this salt artificially.

Profits from these processes (in France.)

From the contents of this report, it is seen that the memoir of Citizen Curaudau contains the results of experiments made on a large scale, highly interesting to manufacturers and merchants, and that, in this view, it will be useful to circulate them through the channel of the scientific Journals. We are therefore of opinion, that the class will give much credit to



Citizen Curaudau for having published the result of his observations, and that it ought to encourage him thus to turn the product of his useful labours to the advantage of his country.

Done in the class of physical and mathematical sciences, the 11th Fructidor, in the year IX.

Signed, GUYTON and VAUQUELIN.

The class approves the report and adopts the conclusions.

#### XIV.

*Letter from Mr. JOHN FARCY, concerning the great Fiery Meteor of Nov. 13, and on other Subjects.*

To Mr. NICHOLSON.

SIR,

Correct date of  
the appearance  
of the fiery me-  
teor in Novem-  
ber last.  
It was on the  
13th.

THE great value of your Journal; as a record of facts connected with science, induces me to request you will correct the errors which have unfortunately crept into your account of the meteor of the 13th ult. which is described, in the second line of the title, and in the first line of the account at page 279, to have happened on the *fifth*; and also in the title of the cuts in the next page, where it is described as an event of the *sixteenth*. I am much concerned that your friend who saw this interesting phenomenon over St. Ann's church-yard, did not give an estimate of its elevation, or of its nearest approach to the zenith, or of the number of seconds during which the light occasioned by it continued.

Inquiries about  
its course, &c.

I was not so fortunate as to see it myself, or since to meet with any scientific person who did, but on the following evening, I enquired minutely of four or five watchmen of St. Martin's and St. Margaret's parishes, who were in different places at the time in their way to their respective watch-houses; I went with each of these persons to the spot, and heard their relation of every circumstance which they saw; one of these persons being just by the Horse-Guards within the Park, and another in the front of the Admiralty, agree in fixing the time at about twenty-nine minutes past eight by the Horse-Guards clock; from their comparison of its height with the seven stars then visible (though it was rather hazy) I am inclined to think that

that it passed within about  $30^{\circ}$  of the zenith, to the South of it, and in a direction from E S E. to W N W. by causing these persons separately to count one, two, three, &c. as long as they thought the light lasted, while I looked at my watch, I conclude its light lasted but five seconds, which agrees very well with the two or three seconds in which it was strong enough to affect persons in a room, as mentioned by you at page 279.

Greatest altitude about  $60^{\circ}$ , and course W.S.W.

During the very heavy fall of snow on Monday evening the 5th Inst. as I was returning home from Drury-Lane Theatre, I saw at distant intervals, four or five sudden flashes of light, which attracted the notice of many persons near me, and was concluded to be lightning: has lightning been observed during the formation of snow in the atmosphere? The air felt rather warm at the time.

Flashes of light during a fall of snow.

Your candour will, I trust, excuse me in lamenting the too concise descriptions given, at pages 218 and 219, of the syphon, and of the substitute for a fly wheel; it has not appeared evident to me that the syphon would act at all, without a stop cock below the funnel, *Fig. 3*. In line 7 of page 219 the printer has evidently made some mistake, and what is given below in explanation of *Fig. 2*, is so far from evident, that I hope you will at your leisure give a more detailed account. In page 220, last line but one, it should refer to *Fig. 4* instead of 3.

Observations upon some articles in this Journal.

You will greatly oblige me by giving a place in your Journal to a paper of Mr. Playfair's in the *Edin. Trans.* vol. V. 1802, containing Theorems on the Figure of the Earth. And to a paper in the *Trans.* of the Linnean Society, describing an extraordinary appearance of the layers of flint in the chalk strata of the Isle of Wight. I am in haste,

Valuable works indicated.

Sir,

Your obedient humble Servant,

JOHN FARCY.

No. 12, Crown-Street, Westminster,

Dec. 15, 1803.

# REPLY. W. N.

Having received Mr. Woulfe's communication late in the month, I gave the account nearly in the concise words of the

F 2

inventor.

inventor. I will take an early opportunity of speaking more fully respecting it. In Mr. Howard's syphon the funnel part may in some measure be considered as a substitute for the side suction pipe. In large syphons neither will operate unless there be a stop cock at the bottom of the outer leg: in small instruments, such as that in the drawing, the stoppage is made by the finger.

The principal intention of a periodical journal being to publish in the form of a book such essays as from their conciseness might neither be circulated nor preserved adequately to their merits, it seems to follow that larger works must for the most part be exceptionable from the space they occupy. The excellent treatise of Professor Playfair being in English, and of considerable length, has not yet been reprinted in our work, chiefly for the latter reason; but I will avail myself of the obliging indications of Mr. Farcy to re-consider both the works he mentions.

## XV.

*Method of restoring rancid Essential Oils. By J. B. DE ROOVER.\**

Process for purifying rancid volatile oils.

**DURING** my long and frequent experiments on essential oils, I have discovered a method of restoring them, when rancid, which appears to possess great advantages. The experiment which led me to it is as follows:—

One sixteenth of ether added to the oil, and left in digestion; five pints of alcohol added. Distillation on the water bath. Most of the oil remained behind.

I mixed one ounce of sulphuric ether with sixteen ounces of oil of peppermint slightly rancid; I left this compound in digestion for twenty-four hours, and then added five pints of spirits of the same mint. The solution was complete. I filtered the liquor and distilled it over the water bath. I expected that nearly the whole of the oil would have passed over in the distillation along with the alcohol and the ether; but, on opening the still, I was much surprized to find the greater part of the oil swimming on the surface of the residual fluid. To ascertain the quantity of oil which had passed over, I precipi-

\* From the Journal of Van Mons (to whom it was communicated by the author,) V. 107. No. 13.



tated it from its solution in alcohol by water, according to my method, and four ounces and a half of oil were separated as transparent and colourless as water.

Oil was separated from the alcohol by adding water.

I then poured the residuum of the still, together with the spirit and the water of precipitation, after having separated the oil, into an apparatus over a naked fire, and distilled a second time. I had the satisfaction to find a spirit rise, the water of which, that came over at the same time, deposited an oil as perfect as before. I repeated this cohobation by pouring the mixture of spirits and water on the remaining oil through the neck of the alembic until I had collected in all twelve ounces and a half of oil as fragrant and free from rancidity as the freshest and best oil of peppermint.

Second distillation of all the products.

Cohobation and re-distillations,

gave a pure oil.

I rectified the spirits and judged by some trials that it might contain rather more than one ounce and a half of oil.

Some oil in the alcohol.

What remained in the still was a thick adhesive matter of the nature of turpentine. I boiled it with water, and obtained ten gros (drams) of a sort of dry turpentine similar to resin.

Resinous matter in the still.

It is difficult to ascertain in what manner the addition of alcohol, or ether, contributes to the restoration, and in some measure the regeneration of so large a quantity of essential oil, which had lost the greatest part of its peculiar odour.

Theoretical speculation.

Formerly it would have been said, that the spiritus rector, with which the alcohol of peppermint was impregnated, had produced this effect; but now when we know that the smell of an essential oil does not depend on a peculiar principle, but on a constitution proper to the oily compound, we cannot admit that the oil of the alcohol had rendered any particle of the dis-  
aromatized oil odorant; but we must suppose that the ether or the alcohol are in their decomposition identified with the substance of the oil.

In fact, the alcohol, and more especially the ether, contains as a constituent part hydrogen, which is the base of almost all the odours. Alcohol, ether, and the essential oils, are equally disposed to undergo an alteration in the relative proportion of their principles. There is consequently no obstacle to our admitting this concurrence of chemical operation for the restoration which I have obtained of the oil of *mint*.

Another circumstance tends to support this explanation. I at first attributed the oil which I obtained in the second operation to the difference of temperature of the bath, and of the  
naked

Theoretical speculation on the purifying of volatile oils.

by naked fire. But the oily products of the subsequent distillations being all made at the same temperature, left me no cause for hesitation in explaining the effect by a successive re-action of the ether or the alcohol on the hydrogenated oil.

A third circumstance in confirmation of this opinion is, that the quantity of alcohol appeared to me to be sensibly diminished, and that in the smell of the oil, which was perfectly pure, I did not find the least trace of that of ether, which is so easily distinguishable. It is true that during the repeated distillations, the ether might have been dissipated as well as the portion of alcohol, which constituted the difference between the original and subsequent quantities of that liquid.

It remains, nevertheless, to be explained how a principle of the alcohol, by assimilation with the principles of the oil, should produce precisely the proportional compound which forms the species of oil on which it re-acts. To this it may be answered that the base of this oil is only disposed to take that principle which shall replace it in its original state.

But even supposing this explanation to be unfounded, and that the influence of the ether and the alcohol shall appear to be merely concurrent circumstances for producing a separation of the particles of oil and restoring its qualities, it is nevertheless certain, that the same effect has not hitherto been produced by any other means, and that my experiment furnishes us with a valuable method of restoring to commerce an immense quantity of essential oil, which would otherwise be of no value.

Second experiment.

P. S. Since the above mentioned experiment, I have made another trial with an oil which flowed as thick as turpentine, and had no smell by which the plant from which it was extracted could be distinguished. I mixed four ounces of this oil with two drams of sulphuric ether, and after four days distilled it with pure water. Two ounces and a half of oil came over as clear and as fluid as spirits of wine, having the best and most decided odour of wild thyme. There remained in the still two ounces of resinous matter of a dark red colour.

In this experiment the ether certainly developed the smell of the oil on which it re-acted, or rather it recomposed the oil by transmitting its hydrogenous principle to the oily base,



## XVI.

*Questions, 1. respecting the Place of the erect Image behind a Concave Speculum; 2. and of the Image formed by a Concavo-convex Mirror which is not Left-handed, and has the Property of revolving on its Axis along with the Mirror, but twice as fast; and 3. the Figure of the Sky. By a Correspondent.*

To Mr. NICHOLSON,

S I R,

I HAVE a general knowledge of the delightful science of <sup>Questions proposed.</sup> optics, but have no pretensions to an intimate acquaintance with its depths. I have therefore presumed to request you will either give your own sentiments on the following difficulties, or lay them before your readers, some of whom I hope will give a popular explanation of them.

1. We are taught by all the writers on vision that the distance of any object is ascertained from the divergence of the rays from any assumed point on its surface, and that as the sense or judgment refers to the point of divergence, we may have a perception of an optical image by reference to that region or part of space from which the rays in their last course are directed, whether they have in absolute fact proceeded from this last point or not. Thus it is that we see an image behind a <sup>General statement of the means by which we infer the positions and distances of visible objects.</sup> looking glass though the rays have never actually proceeded from that image; but only move in the same directions as if they had done so. If the mirror be convex, the image will be nearer to its surface, because the rays will be more scattered and divergent; and seem to have come from a nearer point. And parallel rays are said to come from an object infinitely distant. <sup>Looking glass.</sup>

Now Sir, my query respecting the concave mirror is this:— <sup>Phænomena of the images in a concave mirror.</sup> When I hold my face close to the surface, the image and object touch each other. If I recede, the erect image recedes as the rays become less and less divergent. Every one knows that when I am at the distance of half the radius, the rays are returned parallel, and consequently the image is then infinitely distant. When I move farther off, the rays become convergent, and therefore in a certain sense may denote an image farther off than at an infinite distance; but I have no wish to discuss this point. When I am at the center of the sphere all is



confusion, and beyond that point I begin to discern the inverted image that hangs in the air, and is formed by the divergence of the rays from their focal interfections.

Apparent recess  
and approach of  
the erect image  
in a concave  
mirror.

Such are the facts, but not the whole of the facts. For I am very sure, that after the first receding of the erect image, it comes to a place where it is stationary, and then instead of remaining at any situation that may be called an infinite or more than infinite distance, *it comes forward again* and continues to advance till it is lost in confusion.

Difficulty stated.  
Why does the  
image advance?

Perhaps I may not have expressed myself with all the clearness I could wish; but I think I have shewn a difficulty and the reason why it puzzles me. The true science of nature must be adequate to the explanation of all phenomena, or at least it must not be contrary to any one of them. What is the solution of this?

Optical mirror at  
Merlin's.

2. There is an optical mirror at Merlin's Exhibition in Hanover-street, in which you see your face perpetually changing from one distortion to another, arising from its greatest length varying through all its several diameters. So that the face becomes absurdly long, crooked, broad, &c. If you move farther off, the face has its natural aspect, but is seen turning round its center and successively assuming the erect and inverted positions, and every intermediate degree of obliquity.

Singular distortions,

and revolution of  
the image.

A polished button  
of the same  
figure.

While these phenomena, produced by an apparatus concealed in a box, employed my occasional reflections, I happened to see a polished metallic button in the shops, which upon examination proves to be a mirror of the same kind as the larger one used at Merlin's. If it be considered in the direction of one of its diameters, it is a concave speculum; but in the direction of another diameter at right angles to the former, it is convex: and the radius of each curvature is about one inch. It is the same figure as a turner might produce in the lathe by using a chisel with a circular edge of one inch radius to scoop out a portion of a revolving piece till its smallest diameter, or calliper, became equal to two inches. Its effects are,

Particular description of the  
images produced  
by the button.

When the eye is brought very near the polished surface, it appears elongated in the direction of the diameter or chord, or of the concavity, and shortened in the diameter at right angles to that diameter, and if the speculum be turned on its axis, the image is affected with the strange undulation before mentioned, and undergoes all the variations twice in one revolution

Undulation.

volution of the mirror. But when the eye is at a considerable distance, a small perfect image is seen, which when the diameter of concavity is horizontal, is erect but not left handed; that is to say, if you move your right hand, the image also moves its right hand, instead of the left, as happens in a convex and a plain mirror; when the diameter of concavity is vertical, the image is inverted, but still right handed; and in the other positions of the mirror it takes every possible situation of obliquity, making two complete revolutions for one of the mirror itself. The image is larger the nearer the object, and the erect image is a little broader than the inverted; this deformity being more considerable the larger the image.

Now Sir, my simple question is, *Where is this image?* Is it before the mirror, according to the property of the convex, or behind it, according to that of the concave. I thought to have discovered this by the solar focus, or by applying a magnifier to the small image, but neither method answered my purpose, and nothing within the small extent of my knowledge has enabled me to solve it by reasoning.

3. The enlarged appearance of the moon near the horizon, and the flat vault presented by the concavity of the heavens, have been explained by various allusions to the diminished light after passing through a portion of the atmosphere, and its direction over a long row of objects, such as trees, houses, &c. Something seems to be wanting in these accounts. Neither the sun or moon are ever supposed to be more distant or larger when seen at considerable elevations through clouds or mists; and the sky seems flat, and these luminaries enlarged, whether the prospect be over woods, lawns, naked plains, or the calm sea. Besides which, I have no hesitation in insisting that the sky appears loftier on some days than on others, and even at the same instant of time on different sides. If this be no error, what are the causes of the appearance or the conviction produced in our minds?

I am, Sir,

Your obliged reader from the first,

R. B.

I believe my correspondent would have vanquished his difficulties, with a little more consideration. If the explanation be not given by some of my correspondents, I shall endeavour to give the replies he requests in our next.

W. N.

A New

## XVII.

*A New Method of preparing the pure Gallic Acid.**By M. L. SCHNAUBERT \*.*Preparation of  
gallic acid.

FOUR ounces of finely powdered galls are infused in water slightly alkalized with potash; the infusion which is of a very deep colour is filtered, and a solution of nitrate of tin is dropped into it, until no more precipitate is formed. The free acid of the liquid is then saturated with potash, taking care not to add the alkali in excess, and the precipitate is separated by filtration. The filtered liquid is then precipitated by acetite of lead, the weight and concentration of which is known. A greyish white precipitate is thus obtained, which is to be digested with diluted sulphuric acid, the proportion of the acid contained in which, must be to the concrete acetite of lead as one to four. After a sufficient digestion, the liquid is carefully filtered and evaporated, in order to afford crystals. If the gallic acid thus obtained, should contain a small quantity of sulphuric acid, it may be deprived of it by digestion, with gallate of lead.

In order to be certain of the quantity of sulphuric acid necessary for the decomposition of a given quantity of gallate of lead, I previously dissolved four ounces of acetite of lead in sixteen ounces of water, and diluted one ounce of sulphuric acid in four ounces of water. By mixing these two liquids to afford the perfect precipitation, the requisite proportion was easily found. I must here observe, that it will be better to keep the gallic acid in a liquid state than to reduce it to crystals, because evaporation always renders it more or less brown, I am firmly persuaded that the gallic acid possesses the property of becoming brown by the action of light, at least, if in contact with the atmosphere. For if gallic acid be sublimed perfectly white, and then dissolved in water, it will be found after some time to have undergone this kind of alteration. M. Bucholt has made a similar remark relative to the same fact.

\* Tromsdorff's Journal der Pharmacie, vol. ii. p. 61.



## SCIENTIFIC NEWS.

*Reward of Twenty Pounds for the Artificial Production of Palladium.*

THE following is a copy of a paper received by me under cover, by the two-penny post. It is written in the same hand as a note which covered a small piece of the palladium mentioned to have been received by me last Midsummer. (See *Philos. Journal*, June, 1803. Vol. v. p. 136.) Upon inquiry, I find, that Mrs. Forster has received the sum of 20 £. with instructions conformable to this paper. This original is cut indent-wise on the margin, and has part of a manuscript flourish or paraph on each border, but no signature.

(COPY.)

*December 16, 1803.*

SIR,

AS I see it said in one of your Journals, that the new metal I have called palladium, is not a new noble metal, as I have said it is, but an imposition and a compound of platina and quicksilver, I hope you will do me justice in your next, and tell your readers I promise a reward of 20 £. now in Mrs. Forster's hands, to any one that will make only 20 grains of real palladium, before any three gentlemen chymist's you please to name, yourself one if you like.

It is insisted that palladium cannot be formed by art; and a reward is offered for any process to that effect,

That he may have plenty of his ingredients, let him use 20 times as much quicksilver, 20 times as much platina, and in short of any thing else he pleases to use: neither he nor I can make a single grain.

Pray be careful in trying what it is he makes, for the mistake must happen by not trying it rightly.

My reason for not saying where it was found, was, that I might make some advantage of it, as I have a right to do.

If you think fit to publish this, I beg you to give the names of the umpires, as I have desired Mrs. Forster to keep the money till next Midsummer, and to deliver it only in case they can assure her that the real metal is made by a certificate signed by you, and by them, on this check.

I hope a little bit of whatever is made may be left with Mrs. Forster.

*Letter*

*Letter from Lalande on the Measure of a Degree of the Earth in Lapland\*.*

The measure of a degree in Lapland by Maupertuis, &c. suspected of error.

Re-measured lately by Swanberg, &c. who found it to be 57209 toises, or 196 less.

The flattening of the earth 1-313th.

ASTRONOMERS have long suspected, that there was some error in the measure of a degree of the earth taken in 1736 in Lapland, by Maupertuis, Lemonnier, Outhier,\* and Celsius, because this measure was greater than it ought to have proved according to all others. Mr. Melanderhielm has had the good fortune to obtain a repetition of this measurement. He informs me, that Mr. Swanberg, and three other Swedish astronomers, have found the degree to measure 57209 toises in the latitude of  $66^{\circ} 20'$ , which are 196 toises less than by the French measurement, and gives for the flattening of the earth  $\frac{1}{313}$ . This agrees better with the other comparisons, and teaches us, that the figure of the earth is not so irregular as was supposed, from the degree measured in the north.

Mechain gone to measure a triangle terminating at the balearic isles.

Mr. Mechain set off on the 26th of April for Spain, whither he is gone to measure a triangle of 93000 toises, terminating at the Balearic islands, which will complete the grand and important measurement of the meridian, executed a few years ago by Messrs. Mechain and Delambre. He is accompanied by Messrs. Le Chevalier and Dezauche, and his youngest son. This measurement is very difficult, but no person is more capable of surmounting difficulties than Mr. Mechain, and we shall have the 54th degree in the middle of the whole arc measured by Frenchmen.

New metal.

DESCOTILS has discovered a new metal in the ore of platina, it is presumed that palladium may be an alloy of this new metal with mercury.

A blue equal to ultramarine.

Thenard has discovered a blue equally beautiful, and as fine as that of lapis lazuli, or ultramarine.

Prize of galvanism deferred.

The National Institute has declared, that the galvanic prize will not be given this year.

*These three articles are from the Journal of Van-Mons.*

\* Journal de Physique, May, 1803, p. 400.

*Letter*

*Letter from Professor PROUST to J. C. Delam  therie on a dangerous fulminating Powder \*.*

IN one of my late lectures, I made an experiment that might have been attended with serious consequences to some of my auditors. Dangerous experiment.

It is, perhaps, no exaggeration to say, that the mixture of oxygenated muriate with arsenic, takes fire with the rapidity of lightning.

I am accustomed to compare the quickness with which different kinds of powder burn, by setting fire to them in cannons of equal diameter, passing each through a piece of cork placed in water, so as to sink in it almost their whole length. Mode of comparing explosions with that of gun-powder.

In this manner I was desirous of burning the arsenical mixture, by the side of an equal quantity of gun-powder, in order to compare their flame and continuance; but I had scarcely time to draw back a little after setting fire to the two cannons, when the first exploded and burst, breaking the glass vessel, and throwing about the water in all the radii of the surrounding sphere. Though the mouth of the tube and the escape of the flame were perfectly free, the cannon was burst from top to bottom, and spread out as flat as a card. A mixture of oxygenated muriate and arsenic burnt with the rapidity of lightning, burst the cannon that contained it, rendered it quite flat, and threw about the water in which it floated in all directions.

This powder is so violent in its effects, that I conceive it would be very dangerous, to attempt to make any use of it. Its effects are too dangerous for use.

If two long trains be made on a table, one of gun-powder the other of this mixture, and they be in contact with each other at one end, so as to be fired at the same time, you will see with great surprize, that one disappears like a flash of lightning, while the other seems to burn with extreme slowness. A long train of it disappears like lightning.

*Letter from a Correspondent on the Expediency of converting Foreign Weights and Measures into English.*

To Mr. NICHOLSON.

SIR,

THE pleasure which your readers receive from the perusal of your Journal is so much abated by the obscurity arising from the names of the French weights and measures, that I



wish to suggest to your consideration, that a reduction of them to English terms, would be to you comparatively a small labour, in proportion to what it would be to the generality of your readers individually.

Your labours tend to the useful purpose of communicating science and general knowledge to the public of this country. To facilitate the acquirement of such knowledge, it should be communicated in the terms most easily intelligible to the people; not in such terms as require algebra, or a process in the Rule of Three, to give a precise idea of every measurement. As your Journal becomes more extensively circulated, there arises a stronger claim upon you from the public, in favour of the many who are too indolent, too busy, or too ignorant to expound for themselves this species of hieroglyphics.

I am, Sir,

Your constant READER.

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#### ANSWER.

The rule proposed to be followed in all our translations, though perhaps not precisely, and on every occasion, adhered to, is, that wherever the foreign denominations of weight or measure are mere indications of the proportional quantities, (and may be considered as pounds, ounces, or grains, or as yards, feet, or inches all through,) the original names are used without embarrassing the subject with any reduction into fractional quantities: In every other case the reduction is, or ought to be made.

While I express my acknowledgments to this reader for his advice and remarks, I beg leave to say, that I heartily subscribe to his opinions as to facilitating the communication of knowledge, by the simplest methods. It is my wish in the side notes, and by every other means, to shorten the time, and assist the inquiries, of those whose other duties forbid the indulgence of deep or continued researches.

W. N.

ACCOUNT

## ACCOUNT OF NEW BOOKS.

*A System of Theoretical and Practical Chemistry, in Two Volumes with Plates.* By FREDERICK ACCUM, Teacher of Practical Chemistry, Pharmacy and Mineralogy, and Chemical Operator to the Royal Institution of Great-Britain. Octavo, Two Volumes, 733. and full Index and Contents, with Five Copper Plates. Accum's system of chemistry.

THIS work which, is dedicated by permission to the managers of the Royal Institution, and patronized by a very respectable list of subscribers, is drawn up for the use of such as are unacquainted with chemical science. The author, therefore, begins by describing such experiments as are best adapted to exhibit the general nature of chemical action. The laws of affinity, and also those which govern the phenomena of heat and light, are stated by the method of synthesis, and illustrated by experiment. And these are succeeded by the classification of bodies; namely, first the principles undecomposed, then the characteristic properties of the gases, and these are succeeded by the metals, alcalis, earths, &c. The display of these, as to their properties and habitudes, by an orderly combination of general narrative and reasoning, with particular facts, experiments or, operative processes, constitute the business of the present work, which will doubtless prove acceptable to the numerous cultivators of the science of chemistry.

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*A practical Essay on the Analysis of Minerals, exemplifying the best Methods of analysing Ores, Earths, Stones, Inflammable Fossils, and Mineral Substances in general.* By FREDERICK ACCUM, Teacher of Practical Chemistry, Pharmacy, and Mineralogy. London, 12mo. 183 pages. 1804. Accum's analysis of minerals.

THIS active and indefatigable lecturer, has been requested by his pupils and auditors, to draw up a set of concise directions, to enable any person not intimately acquainted with analytical chemistry, to examine such unknown minerals as he may meet with, and readily ascertain their composition.

In

In the accomplishment of this object he has exceeded his first intention, and has not only given clear, and rather comprehensive instructions for analysing all the different genera, and principal species of the bodies mentioned in the title, and commonly met with, but has also given examples of analysis of the rarer or less abundant mineral bodies.

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Bossut's history  
of mathematics.

*A General History of Mathematics, from the earliest Times, to the Middle of the Eighteenth Century. Translated from the French of John Bossut, Member of the French National Institute of Arts and Sciences, and of the Academies of Bologna, Petersburg, Turin, &c. To which is affixed, a Chronological Table of the most eminent Mathematicians. 8vo. 566. p. London, 1803. One Vol. Octavo.*

THIS justly esteemed work is well translated under the inspection of Mr. Bonnycastle, of the Royal Military Academy at Woolwich, who has added an introductory Preface, and the Table mentioned in the Title Page.



A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

FEBRUARY, 1804.

ARTICLE I.

*Description of an Apparatus, by which the Effect of Atmospheric Pressure in supplying Worm-Tubs and other Vessels with Water on the Syphon Principle, may be secured against any Interruption to be caused by the extricated Air. In a Letter from Sir A. N. EDELCRANTZ, Counsellor of the Chancery, and Private Secretary to the King of Sweden, Member of the Swedish Academy, &c. &c.*

To Mr. NICHOLSON,

MY DEAR SIR,

ON my late return from an agricultural tour in Scotland, I find you have in your excellent Journal, honoured me with the mention of some ideas of mine, respecting the application of syphons to raise water, with less expence than usual, for coolers in distilleries, condensers of steam-engines, &c. The use of syphons is so well and so generally understood, that though the most obvious things do not always present themselves first to the mind, it would have been surprizing if the same notion had not likewise occurred to others. In fact, I have since learned, that a person in America, more than twenty years ago, obtained a patent for using the syphon to cool the worm-pipes in distilleries, and though I neither know his name, nor the specification of his invention, I presume it to

Introduction:  
respecting  
former endeavours to supply  
worm-tubs, &c.  
with water by  
the action of the  
syphon.

be similar to what I have proposed. In addition to which, the ingenious contrivance of Mr. Howard, published in a later number of your Journal, is also founded upon the same hydrostatical laws. But whatever may be the case in respect to theory, no practical use seems yet to have been made of it, owing most probably to unforeseen difficulties in the execution.

The chief impediment lies in the extrication of air which stops the current.

The principal of these, in my opinion, as was mentioned in the former memoir upon this subject, is the extrication of air from the water contained in the cooling vessel; which air being expanded, partly by its own elasticity under a diminished pressure, and partly by the heat, is accumulated in the upper part of the cooler, and interrupts the two columns of the syphon, so as at last to stop the motion entirely. For though it may possibly happen, by the negligence of workmen, that the worm itself may not be sufficiently found to exclude the external air pressing into the cavity, and introducing itself that way into the cooler, as is stated in the experiment of Mr. Howard; yet I am persuaded that in most instances, this cause may be taken for the real one, instead of the former, which can never fail to act in proportion to the perpendicular height of the cooler, and the heat communicated by the worm. As it is impossible to observe by what actual means the air is produced, unless the whole apparatus were of glass, such a mistake may easily be made. In fact, if metallic pipes could not be made air-tight, even in a bended form, the exhaustions in air-pumps and still more in great condensations\* would be attended with almost insuperable difficulties.

Not probable that this air comes from without.

Plan for extracting it.

For extracting the air disengaged from the water itself, during the action of the syphon, which in a few hours time would necessarily interrupt its motion, I have employed a few hours of leisure constructing the following means, which I beg leave, Sir, to communicate to you; without pretending to claim a greater portion of your attention than their remote utility may appear to deserve.

\* In the experiments I made some years ago, with an apparatus constructed for condensing different gases and for producing a stronger artificial cold, than by any means employed before, I compressed air in brass pipes more than to  $\frac{1}{25}$  of its original volume, which if we suppose the cooler to be 15 feet high, is more than 50 times the difference between the external and internal pressure of air in the cooling worm of the new apparatus.

On

On the top of the cooling vessel A, *Fig. 1. Plate V.* let a small vessel B, called the *air-vessel*, be placed, communicating with A by a cock. With the top of B let another vessel C, called the *water-vessel* be connected, having a cock C. These cocks have two parallel openings each, one for letting out air, the other for letting in water at the same time to take its place. Suppose these three vessels to be filled with water and the syphon in action, the cock B open and C shut; then all the air produced in the cooler will rise through B and fill the air-vessel; and in the mean time the water will descend. The air-vessel being filled with air, let the cock B be shut, and C opened; then the water from C will descend, and the air will escape into the atmosphere. By this alternate motion of the cocks, the air disengaged within the worm-tubs will be continually extracted, as long as any water remains in C.

The principal object then must be first to supply the water in C, either by manual labour, or by a part of the mechanical force employed to fill the artificial reservoir for the lower branch of the syphon; and secondly, to direct the alternative opening of the cocks, by a self-regulating mechanism; which may be effected in the following manner.

*Fig. 2. Plate VI.* represents a section of the *air-vessel* B, B is the air and water-cock, communicating with the worm-tub or cooler below. A is the other cock for the same purpose, communicating with the water-vessel C. A and B being connected by a metallic rod K L, their motions are simultaneous, and the perforations are so placed, that when the two holes of one cock are open, those of the other are always shut. E is a ball of lead or other weight, fixed on the lever G U, connected with the quadrant F G, and the lever G H, the whole turning through one quarter of a circle round the centre U; *mn* is a part of a cog-wheel fixed to and sliding along the quadrant G F, acting upon the teeth of the cock A, and forcing it, together with the cock B, one way or the other, as itself is moved by the levers U F or U G, either in the direction from F to G, or from G to F. This alternate motion is produced by the floater P O Q R, (of which a plan on a diminished scale, is represented in *Fig. 3.*) having in its middle and hollow part a kind of fork O, through which passes the metallic rod H I, connected with the lever H F, at the end of the quadrant G F. This floater in its ascension

A vessel filled with water, with a cock at top and another at bottom, is connected with the upper part of the worm-tub. When the lower cock only is opened the extricated air ascends, and water descends. When the upper cock only is opened, the air escapes, and other water fills in from above.

The upper water must be supplied; and the cocks must work without attendance.

The cocks are worked by a float: which when at lowest shuts the under and opens the upper cock; and when at highest does the contrary. The action is made sudden by a tumbling weight;



lifts the little ball H at the end of the lever, and consequently the weight E; and in descending it acts upon the ball I, and forces the weight E in the contrary direction. A side view of this apparatus is presented in *Fig. 4.* where the double openings in the cocks A, B, and the manner of fixing the weight E with its levers, are more distinctly seen.

In *Fig. 2.* the air-vessel is supposed half filled with air transmitted from the cooler. The cock B is shut, and the cock A open, admitting water in place of the air. The floater is ascending, and when the fork O touches the ball H, the weight E is lifted, the quadrant begins to move from E to G, but the sliding part *mn* remains in its place till the weight E, having passed the perpendicular and falling over the other side, brings the point F in contact with *n*, and forces at once both cocks in the contrary direction; by which A being shut and B open, the air from the cooler rises, and the water descends. The floater consequently sinks till it presses upon I, drawing the weight E in its first situation, and turning the cocks again. This process will evidently continue to alternate.

Every mechanical reader will see the necessity of turning the cocks in this apparatus by a sudden stroke. For if the floater were to act directly and constantly upon them; there would be a certain moment when both would be shut, and consequently the motion would cease.

Another method  
by chains.

Instead of the slider *mn*, the same effect may be produced by two chains, (*Fig. 5.*) on the ends of the levers FG, connected with the cock A; the other apparatus remaining as described above. This method may even in some respects appear more simple; but I have described the other partly because I apprehend the contrivance to be new, and partly because the use of chains may in some cases be liable to objections.

An air-pump  
would be the  
simplest organ.

But I cannot avoid remarking that the most simple means of all, and perhaps the best, on a small scale, would be an *air-pump* fixed on the top of the cooler, and connected with the pump that supplies the artificial reservoir. And though the force required for extracting the rarified air, must be in proportion to the perpendicular height of the syphon; yet it cannot be more than what is required to raise a quantity of water necessary to fill the place of the air, as in the former

apparatus. When it has been found by experiments, how much air is extricated from a given quantity of water, running with a known velocity, under a given diminution of pressure and heat; the size and power of the air-pumps may be determined so as to act with full advantage, as long as these circumstances remain unvaried. But if they happen to change, the air-pump will either work too much, and expend an unnecessary force, by extracting water after the air is exhausted; or too little, by extracting the air more slowly than it is produced. On the contrary, it seems that the floater, if the vessels and cocks be of sufficient size, can never deliver more or less than is exactly required.

This consideration, as well as the possibility of a constant supply of water for small vessels, without any pumping at all, may in many cases allow the first apparatus to be used with advantage, and even render it preferable, especially in elevated syphons and great rarefactions, as most commonly occur in practice, when it would not only be very difficult to keep the air-pump tight without great friction, but a quantity of vapour from the heated fluid would be expanded. This increases the difficulty of the air-pump, but in the other contrivance it is partly condensed by the cold water in the air-vessel trough, which it is obliged to pass.

I am with great esteem, My dear Sir,

Your obedient humble Servant,

A. N. EDELCRANTZ.

## II.

*Enquiries concerning the Nature of a Metallic Substance lately sold in London as a new Metal, under the title of Palladium.*

By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A. \*

ON the 29th of April I learned, by a printed notice † sent to Mr. Knox, that a substance, which was announced as a new metal, for sale.

\* From the Philos. Trans. 1803.

† "Palladium, or new silver, has these properties amongst others that shew it to be a new noble metal.

"1. It dissolves in pure spirit of nitre, and makes a dark-red solution. 2. Green vitriol throws it down in the state of a regulus from this solution, as it always does gold from aqua regia. 3. If



metal, was to be sold at Mr. Forster's, in Gerrard-street. The mode adopted to make known a discovery of so much importance, without the name of any creditable person except the vender, appeared to me unusual in science, and was not calculated to inspire confidence. It was therefore with a view to detect what I conceived to be an imposition, that I procured a specimen, and undertook some experiments to learn its properties and nature.

Its general properties.

I had not proceeded very far, when I perceived that the effects produced by this substance, upon the various tests, were such as could not be referred, *in toto*, to any of the known metallic substances. I immediately returned to Mr. Forster, and became possessed of the whole quantity which had been left in his hands for sale. I could not obtain any information as to its natural state, or any trace that might lead to a probable conjecture.

Laminated small pieces.

The substance had been worked by art: it had been rolled out in flattening-mills; and was offered for sale in specimens consisting of thin laminæ. The largest of them were about three inches in length, and half an inch in breadth, weighing on the average 25 grs. and were sold for one guinea. The other laminæ were smaller, in proportion to the price.

Polish, like platina: Elasticity moderate: Flexibility considerable. Specific gravity, differing in the specimens.

Subjected to the same treatment as platina, to procure a polished surface, palladium assumed an appearance scarcely to be distinguished from that metal. The laminæ were not very elastic, but were very flexible, and could be bent several times in opposite directions without breaking. The specific gravity, I found to differ not a little from that which is stated in the printed notice, and to vary considerably in different specimens. Some pieces of this substance were as low as 10,972, while others gave 11,482.

“ 3. If you evaporate the solution, you get a red calx that dissolves  
“ in spirit of salt or other acids. 4. It is thrown down by quick-  
“ silver, and by all the metals but gold, platina, and silver. 5. Its  
“ specific gravity by hammering, was only 11.3; but by flattening,  
“ as much as 11.8. 6. In a common fire the face of it tarnishes  
“ a little, and turns blue, but comes bright again, like other noble  
“ metals, on being stronger heated. 7. The greatest heat of a  
“ blacksmith's fire would hardly melt it; 8. But if you touch it,  
“ while hot, with a small bit of sulphur, it runs as easily as zinc.  
“ It is sold only by Mr. Forster, at No. 26, Gerrard-street,  
“ Soho, London; in samples of five shillings, half a guinea, and  
“ one guinea each.”



The effects of galvanic electricity upon palladium, were the same as upon gold and silver. No oxidizement of the substance took place; but oxygen gas was emitted; during the whole time it formed a part of the galvanic circle in action.

Galvanic effects, as gold or silver.

A lamina of this substance being exposed to the blowpipe, the side removed from the immediate action of the flame became blue; but the temperature at which this colour was produced, exceeded that at which steel begins to lose the tinge it had received at a lower heat.

Became blue from heat.

I exposed palladium, in an open vessel, to a greater degree of heat than that which can melt gold. No oxidizement ensued; and, although the metallic slip was extremely thin, no appearance of fusion took place, even at the edges or corners. Upon increasing the fire considerably, I obtained a melted button; but I cannot estimate the degree at which the fusion was effected.

No oxidizement by strong heat. Fusion more difficult than that of gold.

The button, by this treatment, had lost a little of its absolute weight; but its specific gravity had increased from 10,972 to 11,871. It was of a grayish-white. Its hardness was rather superior to that of wrought iron. By the file, it acquired the colour and brilliancy of platina. It was malleable to a great degree. Its fracture was fibrous, and in diverging stræ, which seemed to be composed of crystals; the surface of the button also, when seen through a lens, appeared to be crystallized.

Fused button rather lighter, more dense, harder than iron, malleable, fracture fibrous crystallized.

Palladium very readily combines with sulphur. I exposed a certain quantity of it to a violent heat, without being able to melt it; and, at that elevated temperature, threw some sulphur upon it. It immediately entered into fusion, and remained in that state until the redness of the crucible was hardly visible in the daylight. The increase of weight in the button of the sulphuret, was such as could not indicate with exactness the proportion of sulphur combined with it; and I was so limited in the quantity of palladium I could obtain on any terms, that I thought it prudent to reserve as much as possible for the investigation of more important properties. Sulphuret of palladium is rather whiter than the substance itself, and is extremely brittle.

Combines and flows with sulphur by ignition. Sulphuret whiter and very brittle.

Palladium, melted in a charcoal crucible, and kept in fusion for fifteen minutes, did not acquire any properties different from those which I have already mentioned, in speaking of the

Continued fusion of palladium in charcoal; no change effect

effect of heat upon that substance. Hence we may conclude, that there is not any action between charcoal and palladium.

Alloy. Palladium and gold; gray and less ductile.

I put equal parts of palladium and gold into a crucible, for the purpose of forming an alloy. The result, owing to an accident, did not weigh so much as the sum of the quantities employed; therefore, the proportions in this alloy were uncertain. Its colour was gray; its hardness about equal to that of wrought iron. It yielded to the hammer; but was less ductile than each metal separate, and broke by repeated percussions. Its fracture was coarse-grained, and bore marks of crystallization. Its specific gravity was 11,079.

Palladium and platina; denser, less malleable.

Equal parts of platina and palladium, entered into a fusion at a heat not much superior to that which was capable of fusing palladium alone. In colour and hardness, this alloy resembled the former; but it was rather less malleable. Its specific gravity, I found to be 15,141.

Palladium and silver; whiter.

Palladium, alloyed with an equal weight of silver, gave a button of the same colour as the preceding alloys. This was harder than silver, but not so hard as wrought iron; and its polished surface was somewhat like platina, but whiter. Its specific gravity was 11,290.

Palladium and copper; harder, dull colour.

The alloy of equal parts of palladium and copper was a little more yellow than any of the preceding alloys, and broke more easily. It was harder than wrought iron; and, by the file, assumed rather a leaden colour. Specific gravity 10,392.

Palladium and lead: more fusible, gray, close grained, and hard and brittle.

Lead increases the fusibility of palladium. An alloy of these metals, but in unknown proportions, was of a gray colour, and its fracture was fine-grained. It was superior to all the former in hardness, but was extremely brittle. I found its specific gravity to be 12,000.

Palladium and tin: grayish, brittle, compact.

Equal parts of palladium and tin gave a grayish button, inferior in hardness to wrought iron, and extremely brittle. Its fracture was compact and fine-grained. Specific gravity 8,175.

Palladium and bismuth; gray, very hard and brittle.

With an equal weight of bismuth, palladium gave a button still more brittle, and nearly as hard as steel. Its colour was gray; but, when reduced to powder, it was much darker. Its specific gravity, I found to be 12,587.

Palladium and iron. Palladium and arsenic.

Iron, when alloyed with palladium, tends much to diminish its specific gravity, and renders it brittle. Arsenic increases the fusibility of palladium, and renders it extremely brittle.

From



From these experiments, we may form the following table, shewing the difference between the true and the calculated mean of specific gravity in the alloys of palladium.

Metals.	Proportion.	Specific gravity by calculation *.	Specific gravity by experiment.	Difference.	Comparison of the specific gravities of these alloys with the computed densities.
Palladium alloyed with	Gold -	uncertain.	11,079	uncertain.	
	Platina -	equal parts.	17,241	15,141	-2,100
	Silver -	equal parts.	10,996	11,290	+ ,294
	Copper	equal parts.	10,176	10,392	+ ,216
	Lead -	equal parts.	uncertain.	12,000	uncertain.
	Tin -	equal parts.	9,340	8,175	-1,165
	Bismuth	equal parts.	10,652	12,587	+ 1,935

I exposed ten grains of palladium to the action of potash, in Palladium fused with potash lost weight and brilliancy. fusion during half an hour. The substance lost its brilliancy, and diminished two grains and a half in weight; these were found in the potash.

The action of soda upon palladium, does not appear to be With soda. quite so violent.

Ammonia, allowed to remain for some days upon palladium, Ammonia in the humid way determines oxidation, and takes up oxide. acquires a slight bluish tinge, and holds a small portion of oxide of palladium in solution. In all these cases, the action of the alkali is promoted by the contact of the atmospheric air, the oxygen of which combines the metal, in favour of the affinity the oxide of palladium possesses towards the alkali.

Some of the pieces of palladium were more easily acted upon by the acids than others; and, in general, those of the greatest specific gravity were the least affected. Upon the whole, however, the following statement may be taken as the average of the habitudes of palladium with the acid solvents.

\* In the specific gravities of the different metals, I have followed the table given in our best elementary work, Dr. Thompson's System of Chemistry,

Sulphuric



Sulphuric acid  
and palladium.  
Weak action:  
beautiful red  
solution.

Sulphuric acid, boiled upon palladium, acquires a beautiful red colour, and dissolves a portion of the substance. The action of this acid is not very powerful, and, upon the whole, it cannot be looked upon as a good solvent for palladium.

Nitric acid more  
active; fine red  
solution.

Nitric acid acts with much greater violence upon palladium. It oxidizes the substance with somewhat more difficulty than it can oxidize silver; and, by dissolving the oxide, forms a very beautiful red solution. If the nitric acid be impregnated with nitrous gas, its action upon palladium is much more rapid.

Muriatic acid.  
Red solution.

Muriatic acid, by being boiled upon palladium for a considerable time, acts upon it, and becomes of a beautiful red.

Nitro-muriatic  
acid is the true  
solvent.

But the true solvent of palladium is nitro-muriatic acid, which attacks it with great violence, and forms a beautiful red solution.

Precipitate from  
acids by alkalis  
or earths, fine  
orange partly re-  
dissolved.

From all these acid solutions of palladium, a precipitate may be produced by the alkalis and earths. These precipitates are, for the most part, of a beautiful orange; are partly redissolved by some of the alkalis; and the supernatant liquor of the precipitate formed by ammonia is sometimes of a fine greenish blue. Sulphate, nitrate, and muriate, of potash, or of ammonia, produce an orange precipitate in the salts of palladium, as in those of platina, when not in too dilute solution; and the precipitates from the nitrate of palladium are in general of a deeper orange. All the metals, except gold, platina, and silver, cause very copious precipitates in solutions of palladium. Recent muriate of tin produces a dark orange or brown precipitate in neutralized salts of palladium, and is an extremely delicate test. Green sulphate of iron precipitates palladium in the metallic state; and, if the experiment succeed, the precipitate is about equal in weight to the palladium employed. Prussiate of potash causes an olive-coloured precipitate; and water impregnated with sulphuretted hydrogen gas, a dark brown one. Fluoric, arsenic, phosphoric, oxalic, tartaric, citric, and some other acids, together with their salts, precipitate some of the solutions of palladium, and form various combinations with this substance.

Such are the principal characters I have found in palladium, examined as a simple metallic body. It does not appear that, in stating any of its properties, except its specific gravity, the printed notice has been guilty of misrepresentation.

From

From these experiments, it would be difficult to say of what metal, or of what combination of metals, palladium consists. We could not suppose gold or platina to be an ingredient in it, as it is in some measure acted upon by sulphuric and muriatic acids, and is wholly soluble in nitric acid. Silver is excluded, by the effect of muriatic acid upon its solutions; as is lead, by that of the sulphuric. Tin, antimony, bismuth, or tellurium, would have left an insoluble residuum with nitric acid. No traces could be found of any of the acidifiable metals; and iron was looked for with particular care, but in vain. In a word, the precipitation by the metals, seems to exclude all those of easier oxidability than mercury; and this we should not suppose to be present, as copper is not in the least whitened, when used to precipitate palladium.

The striking similarity of many of the precipitates of palladium with those of platina, induced me to multiply the comparative experiments; and I constantly observed contradictory facts. The specific gravity, easy fusibility, combination with sulphur, precipitation by green sulphate of iron and by prussiate of potash, together with other effects, were such as I could not reconcile to the known characters of platina; unless I could suppose that a substance did exist, which could totally change its physical and chemical properties, or so disguise them as to render them proof against the evidence of chemical re-agents.

The lightest of the metals is tellurium; yet, in order to produce an alloy of the specific gravity of palladium, (supposing for a moment the real density of the alloy equal to the calculated mean), it would require two parts of tellurium and one of platina; and it is highly improbable, that so large a proportion of tellurium could exist in any mass, without being detected. We have been told of very extraordinary anomalies in chemical affinities, by Mr. Berthollet; and Mr. Hatchett has made us acquainted with some, not less extraordinary, in the properties of alloys. Yet I think we shall cease to wonder at what has been related by these chemists, when we learn that palladium is not, as was shamefully announced, a new simple metal, but an alloy of platina; and that the substance which can thus mask the most characteristic properties of that metal, while it loses the greater number of its own, is mercury.

I confess it was not from an analysis of palladium that I was first led to this result; for I had convinced myself, by synthesis,

Comparison of this body with the other known metals.

It resembles platina in many of its precipitates, &c.

It is an alloy of platina and mercury.

This was synthetically discovered.



Admirable disposal of platina and mercury by which they were presented to each other and united.—Green sulphate of iron was added to a solution of platina; and also to a solution of mercury. No precipitate took place in either. The compounds were then mixed. Platina and mercury fell down together. The compound was fused. It was palladium.

of its nature, and had formed the substance, before I could devise any probable method of ascertaining its component parts,

In reflecting upon the various modifications which substances undergo when in union with each other, and on the variations produced in the laws of affinity by the intervention of new bodies, I was induced to try whether by the affinity of platina with some metal easily reduced, it might not happen that a reduction of both would take place by green sulphate of iron, although no such effect were produced upon each metal when separate. The most likely to succeed, as being most easily reduced, after gold, platina, and silver, was mercury. I poured some solution of green sulphate of iron into a salt of platina, and also into a salt of mercury; no precipitation took place. I united the two liquors; and a precipitate exactly resembling that which is formed by green sulphate of iron in palladium, was instantly formed. I collected the precipitate, and exposed it to a strong heat; and, after repeated trials, obtained a metallic button, not to be distinguished from palladium.

It certainly is one of the most extraordinary facts respecting alloys, that two metals, by their union with each other, should so lose the characteristic properties of each individually, that neither of them can be immediately detected by the usual methods. Nothing but an affinity of the most powerful order could produce such effects. But, to place the metals under the most favourable circumstances for that affinity to exert its influence, and to promote their union, is not the result of common methods. Among a great number which I have tried, many have failed, and none have been attended with uniform success. I have, however, formed palladium by the immediate union of platina and mercury; and, as whatever may place the apparent capriciousness of this combination in a conspicuous point of view is not devoid of interest, I shall describe the means by which I have attempted to produce it, whether they failed, or were attended with success.

Palladium by the immediate union of platina and mercury.

#### SYNTHETICAL EXPERIMENTS.

*Exp. 1.* It was not till after repeated trials of the mode just mentioned, that I succeeded in forming palladium. In many instances, I obtained a button completely melted, of the specific gravity of 13, and sometimes more; not so easily fused by sulphur as palladium; not soluble in nitric acid; and the absolute



absolute weight of which exceeded that of the platina originally employed. But, although this substance was not platina, I could not say it was palladium. The most successful experiment by this method, was attended with the following circumstances. I dissolved one hundred grains of platina in nitromuriatic acid, and then put in two hundred grains of red oxide of mercury, made by nitric acid; but this not being sufficient to saturate the excess of acid, I continued to add more, until it ceased to be dissolved. On the other hand, I prepared some green sulphate of iron, and poured it into a long-necked matraass. I then poured the mixed solution of platina and mercury into the solution of green sulphate of iron, and heated the whole upon a sand bath. In less than half an hour, a copious precipitate was formed; and the inside of the matraass was lined with a thin metallic coat. The liquor was passed through a filtre, which I had weighed; and the precipitate, after digestion with muriatic acid, was well washed and dried. When I had collected as much of this as I could, there remained upon the filtre 12 grains; besides which, I had collected 264, in all 276. The supernatant liquor still contained a portion of mercury, and about eight grains of platina. Therefore, the 276 were composed of 92 of platina, and 184 of mercury. From this it appears, that one hundred grains of platina, can determine the precipitation of near two hundred grains of mercury, by green sulphate of iron; and that, in this proportion, there is a reciprocity of saturation. The 264, collected from the filtre, were exposed to a low red heat, and were reduced to 144. The twelve of the filtre would have given about seven; therefore, the whole would have been 151. The substance was in the form of a fine powder, and had a metallic lustre. It was then put into a charcoal crucible, and fused into a button. This button weighed 128 grains, and with the quantity left on the fibre, would have weighed 135. In this 135, there were 92 of platina; therefore it was composed of about two parts of that metal and one of mercury. It was of the specific gravity of 11.2; was wholly soluble in nitric acid; was easily fused by sulphur; was precipitated by green sulphate of iron: in a word, it was not to be distinguished from palladium.

Experiment 1.  
The most successful experiment of forming palladium by the mode first mentioned.

It contained about two parts platina and one mercury.

*Exper. 2.* As another mode of forming palladium in the humid way, I put metallic iron into a mixed solution of platina and

Experiment 2.  
Mixed solution of platina and

mercury was precipitated by iron. The success was less complete.

and mercury. Both metals were precipitated; and the precipitate was submitted to the same treatment as in the former case; but the success was not so complete. Iron can precipitate either platina or mercury separately; but green sulphate of iron can perform its function only in favour of the affinity of platina and mercury. Their union is promoted by its action; and the effects are, in all probability, simultaneous. The combination of the metals takes place, if I may be allowed the expression, in their nascent metallic state, and in a fixed proportion of mutual saturation. The union of the two metals, therefore, is in the present experiment less intimate, and the button which results from fusing the precipitate, is of much greater density.

Exp. 3. Zinc the precipitant. Incomplete.

*Exper. 3.* The same process was repeated, only using zinc instead of iron, but the result was not more satisfactory.

Exp. 4. Mercury added to solution of platina. Incomplete.

*Exper. 4.* I poured some mercury into a solution of platina, and heated them together for some time. A precipitate took place; but, upon fusing it into a button, I did not find it to be palladium.

Exp. 5. Mixed solutions of platina and mercury evaporated and ignited. The mercury flew off.

*Exper. 5.* I dissolved the same quantities of platina and mercury as in *Exper. 1*, in nitro-muriatic acid, and evaporated those solutions together. I then volatilized as much as I could of the mercury, at a red heat. At the end of the operation, I obtained precisely my original quantity of platina, reduced to the metallic state; but not one particle of the mercury remained along with it.

Exp. 6, 7. Platina and mercury precipitated by phosphate of ammonia. No success.

*Exper. 6 and 7.* The same quantities of platina and mercury, dissolved in nitro-muriatic acid, were precipitated by phosphate of ammonia; and the liquor was evaporated. The residuum, in a glassy state, was exposed to a violent heat in a charcoal crucible; and I obtained a melted button, which weighed more than the original quantity of platina, and was of the specific gravity of 14,5. On account of the easy fusibility of phosphuret of platina, I likewise tried to combine it directly with mercury, but could not succeed.

Phosphuret of platina.

Exp. 8. Platina and mercury precipitated by sulphuretted hydrogen. In one instance palladium.

*Exper. 8.* I precipitated a mixed solution of platina and mercury, by a current of sulphuretted hydrogen gas; and reduced the insoluble powder. After many attempts, in which I obtained buttons of the specific gravity of 14,3 and 14,5, I formed a piece weighing 11 grains, of the specific gravity of 11,5. This



11,5. This last was palladium; but I could not ascertain the excess of weight, as a part of the original precipitate had been lost.

*Exp. 9.* I mixed a solution of muriate of platina with prussiate of mercury, and obtained a slight precipitate. The liquor was evaporated, and the whole residuum exposed to a violent heat. This experiment did not succeed. It was not repeated so often as the others; but I have some reason to think it might be attended with success; for I obtained, in one instance, a few very minute grains, that were soluble in nitric acid.

*Exp. 9.* Muriate of platina and prussiate of mercury. Evaporation and strong heat. Partial success.

*Exp. 10.* I heated some purified platina, in the form of a very fine powder, with ten times its weight of mercury, and rubbed them together for a long time. The result was, an amalgam of platina. This amalgam, exposed to a violent heat, lost all the mercury it had contained; and the original weight of the platina remained without increase.

*Exp. 10.* Amalgam platina and mercury. Heat drove off the mercury.

*Exp. 11.* The best method of forming an amalgam of platina, is that prescribed by Count Muffin Pushkin. I dissolved a known quantity of platina in nitro-muriatic acid, precipitated by ammonia, and evaporated the liquor. The residuum was rubbed for a long time with a great quantity of mercury, and then exposed to a violent heat. Many operations failed; in some, I had a button of the specific gravity of 13,2. In one attempt, I succeeded completely: from 30 grs. of platina, treated as above, I obtained a button weighing 43,5, of the specific gravity of 11,736, which had all the properties of palladium.

*Exp. 11.* Count Muffin Pushkin's amalgam of platina heated gave palladium in one attempt.

*Exp. 12.* I fused together, in a charcoal crucible, 100 grs. of platina, 200 of cinnabar, 100 of lime, and 400 of calcined borax; and obtained a button, which weighed more than the platina, and was of the specific gravity of 15,7. It was not soluble in nitric acid; but combined with sulphur, at a red heat.

*Exp. 12.* Platina, cinnabar, lime and borax fused.

*Exp. 13.* In some experiments I had made, I found that the furnace in which I formed these alloys, was capable of melting platina, without the assistance of any flux except calcined borax. I therefore urged 100 grs. of platina, at a very strong heat; and, when I judged the fire to have attained its greatest intensity, I poured mercury upon the platina, through a long earthen tube that terminated in the crucible,

*Exp. 13.* Platina fused and mercury thrown in. No effect.



and immediately withdrew the apparatus from the fire. No sensible union of the metals had taken place; nor had the platina increased in weight.

**Exp. 14.** Mercury in vapour passed over ignited platina. No effect.

*Exp. 14.* I put 100 grains of platina into an earthen tube, and placed the tube horizontally in the above furnace. At one end of it was a retort, containing 2lbs. of mercury. When the tube was at its greatest heat, the mercury was made to boil; and the entire quantity passed over the surface of the platina, at that temperature. The experiment lasted one hour and a half; but the metals did not seem to have combined.

**Exp. 15.** Platina wire plunged in mercury and heated by galvanism. Little effect.

*Exp. 15.* Mr. Pepys was so obliging as to try the effect of his very powerful galvanic battery, in forming palladium. A piece of platina-wire was plunged into a basin of mercury, and formed part of a galvanic circle. The wire was nearly in fusion; but no combination seemed to take place. The nature of this experiment did not allow of very accurate weighing; but the fused globules of platina did not appear to have acquired the properties that constitute palladium.

**Remarks.** Disposing affinities and assimilation.

Such are the experiments by which I attempted to form palladium. They were chiefly founded upon two principles; disposing affinity, and assimilation. In the one case, I endeavoured to present to the metals that compose it, a substance which, on account of its affinity for some menstruum necessary for their solution, and of their own tendency to combine in the proportions stated in *Exp. 1*, might cause them to unite in the form of an insoluble compound. In the other case, I hoped to assimilate the properties of each, and, by making them something more alike, to place them in the most favourable circumstances for uniting. *Exp. 1*. was founded on the former, and *Exp. 8* on the latter of these principles.

The platina always acquired weight when its properties were thus changed.

Mercury was indispensable.

In many instances, when I did not form palladium, I obtained a metallic button which was not platina; and, when I did so, it always weighed more than the original quantity of platina employed. In repeating *Experiments 1, 2, 4, 6, 8, 11, and 12*, I seldom failed of having such a substance. No effect of this kind took place in any experiment, when mercury was not used along with platina; and the other metals were merely accessaries, in promoting their union and precipitation. This is sufficiently proved, by the uniformity of the results in different processes, whether it was palladium or the substance I now mention which was formed. The chief property which distinguishes

distinguishes the latter substance from platina, is its density. It is not unusual to obtain it of a specific gravity so low as 13; very frequently 15 or 17. In the first experiments, I suspected this lightness to be owing to some air-bubbles; but repeated fusion, and comparative experiments upon platina, soon convinced me of the contrary. The augmentation of weight also, which the platina never fails of acquiring, proves that this metal has combined with some ponderable substance; and, in fact, the result of these operations is, an alloy which is a mean betwixt platina in its pure state and what has been called palladium. It is, consequently, subject to infinite variation. The first effects which mercury produces upon platina are, to render it more fusible, and to diminish its specific gravity. The next new property conferred upon it is, the power of uniting with sulphur; and, lastly, it becomes soluble in nitric acid. It is not however till the specific gravity is below 12, or 12.5 at most, that it has acquired this property; and all these effects follow the direct order of the increase of weight observable in the platina.

It is not very difficult to combine a small quantity of mercury with platina; but, to resolve the problem completely, and to produce an alloy of these metals which shall be of so low a specific gravity as 11.3, and shall be soluble in nitric acid, is not so easily accomplished. From the repeated failures which I have experienced in these operations, I am much inclined to think that the author of palladium has some method of forming it, less subject to error than any I have mentioned. No doubt that perseverance would put us in possession of his secret; but, being prevented by want of leisure from pursuing these researches at present, I have confined myself to establishing the fact, and describing the processes which I have employed.

Having thus acquired a certainty that mercury is a constituent part of palladium, I made some further experiments upon it, with a view to its analysis; but they have not been attended with so much success. It might be expected, from the great number of methods which have failed to form palladium, that many might be found to decompose it when formed. But I have found the converse of such processes as did not succeed in producing palladium, to be ineffectual in destroying the combination.

Mercury first renders platina more fusible and lighter; then renders it combinable with sulphur; and still more renders it soluble in nitric acid, viz. under the specific gravity 12.

The author of palladium probably has some steady method of composing it.

Its decomposition extremely difficult.



## ANALYTICAL EXPERIMENTS.

Analytical experiments not successful.

Excess of mercury added to solution of palladium.

Palladium strongly and durably heated.

Cupellation.

Combustion in oxygen,

by galvanism.

Wonderful differences between mixture and combination.

Affinity not proportioned to the facility of union.

*Exp. 1, 2, and 3.* The converse of the synthetical experiments 1, 2, 3, was made, but without any satisfactory result.

*Exp. 4.* The converse of *Exp. 4* was made without success. I put some mercury into a solution of palladium, and left them together for some time. The precipitate which was formed was palladium, just as it had been used for the operation.

*Exp. 5.* I exposed different pieces of palladium to a very violent heat for two hours. In some, a diminution of absolute weight, with an increase of specific gravity, took place; in others, neither of these effects was produced. The specimens which I had made were chiefly of the latter kind.

*Exp. 6.* Cupellation did not afford any satisfaction respecting the analysis of palladium; but the heat necessary for this purpose is so great, that I could not place great reliance upon this experiment. It is difficult to detach the button from the cupel with accuracy.

*Exp. 7.* I burned some palladium in oxygen gas. A white smoke arose during the combustion, and was deposited upon the sides of the glass jar that contained the gas. But this smoke was palladium, and not the mercury separated from it.

*Exp. 8.* A slip of palladium, which Mr. Davy had the goodness to expose, in my presence, to the action of the strong galvanic batteries of the Royal Institution, burned with a very vivid light, and a white smoke; but no mercury was separated by this operation.

There is not any property of this compound which appears to me so wonderful, as that which is manifested by these experiments. It is a striking proof how unfounded was the opinion of some philosophers, who supposed that the rapidity of combination was a measure of the force of affinity. We do not know of any affinity among chemical bodies which is more powerful than that of platina and mercury appears to be. The obstacles which must be overcome, in order to fix the latter metal, are a proof of this; yet the difficulty of forming this combination to its full extent is extreme. The difference which exists between the compound and its elements, when merely mixed, either in solution or otherwise, cannot be better



better exemplified than by comparing the result of the 5th synthetical experiment, with the difficulty of expelling mercury from the compound.

I must here observe, that all the analytical experiments, and many others, were made, by way of comparison, upon the palladium I had bought, as well as upon that which I had made. But, although I had myself combined the mercury with the platina, and consequently knew it to be in the compound that resulted, I could not succeed in separating it. Neither did the substance described in a former paragraph, as intermediate between platina and palladium, allow one particle of mercury to escape from it, by any process I have yet been able to devise.

The mercury could not be separated from the bought palladium; nor from any of the author's real combinations.

The name of palladium conveys to our mind the idea of something absolute, and therefore incapable of gradation. But gradations in alloys are infinite; and the alloy of platina and mercury is susceptible of infinite variation. Palladium also brings to our recollection a contemptible fraud directed against science: the name, therefore, should not be admitted. I have called it an alloy; for it differs too much from the usual idea we have annexed to the word amalgam, but it accurately corresponds with our notions of the name I have adopted.

Objections to the word palladium.

The facts which I have related in this paper, appear at first sight to have no similar examples in chemistry; and may not gain immediate assent from every person. The philosopher, indeed, will feel no humiliation in being forced to correct or to extend his knowledge; and will not altogether disbelieve a fact, because he can adduce no parallel instance, or because it is not in unison with his received opinions. Such conduct would be raising an insurmountable barrier against the progress of science; it would be setting up our own feelings in the place of nature; and attempting to measure what in itself is immeasurable, by the narrow scale of human comprehension.

These new facts are worthy of entire credit.

But let us not confine our view of the facts and principles that have been mentioned, to this single instance. Let us trace them in a more extended circle; and see whether any thing may be found in nature that can apply to the present subject.

The first prejudice, for such I must call it, against the presence of platina in palladium is, the small density of the alloy. And no doubt it is extraordinary, that a metal the specific gravity of which is at least 22, (Chabaneau says 24,) combined with another the specific gravity of which is nearly 14,

The strange diminution of specific gravity in this compound must not prejudice our belief.

Other similar facts.

should produce a mass of the specific gravity of 10,972; not much more than half of that which calculation would denote, and inferior to either of its elements. In Mr. Hatchett's Paper upon the Alloys of Gold, to which I always refer with pleasure, we find some extraordinary instances of anomalies in specific gravity, both in excess and diminution upon the calculated mean. His experiments have not been doubted; nor can their accuracy be called in question. The principle of deviation in the true and the calculated mean is therefore admitted. Who then can say where this deviation shall end, or mark out limits to the operations of nature?

Particularly the different specific gravities of gasiform water or steam and a mixture of the gases which form it. These are as 7 to 4.

But a no less extraordinary instance of irregular density is daily before our eyes; yet it has not so much as attracted our attention. It is true that it is taken from among the gases. But, if we suppose that we have attained accuracy in experiments upon these subjects, I see no reason to refuse their evidence in this instance. The density of oxygen gas, to that of water is as 1 to 740; and the density of hydrogen gas as 1 to 9792. The mean density of that proportion of oxygen and hydrogen gases which constitutes water, is to that of water as 1 to 2098; or, in other words, water is 2098 times heavier than the mean density of its elements in the gaseous state. But water is only 1200 times heavier than steam, or water in the state of vapour. Therefore, there is a variation in  $\frac{1}{4}$ , of 898, or nearly half, between the density of water and its elements, when both are in the aeriform state. This fact, however, regards bodies only as they remain in the same state, whether of solidity, liquidity, or fluidity. The anomaly is much greater, if we contemplate them as they pass from one of these states to the other. Yet we must not omit the consideration of such a change, in the instance of mercury alloyed with platina; for the former metal, before liquid, becomes solid as it enters into the new combination.

The fixation of mercury is a fact analogous to many others, and ought to excite no prejudice.

A stronger prejudice will perhaps exist against the fixation of so volatile a substance as mercury. It is certain that the labours of the alchemists have thrown some ridicule upon this subject, as a philosophic pursuit. Men of science have long since declined the research; and it is not probable that we are indebted to experiments undertaken in the true spirit of philosophy, for the present fixation of mercury. But, the same cause which induced us to look upon the project as chimerical,

should



should dispose us to admit it when accomplished. Every chemist well knows that similar fixations of volatile substances are not uncommon. If an ore containing sulphur, or arsenic, or antimony, be gently roasted, a great part of those volatile bodies is driven off; but, if a fusing heat be suddenly applied, the mass unites in such a manner that a very small share of them escapes. Mr. Hatchett has instanced an artificial combination of gold and arsenic, from which he could not expel the latter metal, by any degree of heat. Yet arsenic, though less fusible, is not much less volatile than mercury. I will also add a case still more in point; *viz.* the combination of arsenic and platina, which is not to be broken by a fusing heat.

Instances; the volatile bodies sulphur, arsenic, antimony.

An example of this fact, occurs again in water. The liquefaction or solidification of two gases to produce water, by a loss of caloric, never shocks our mind, because it is familiar to us. We cannot say what loss of caloric may be sustained by mercury, in order to unite with platina; or how far the presence of the latter may contribute to expel caloric from the former. We know too, that at any temperature, without the aid of a combustible body, to act as a reductive, we have not been able to disunite the last portions of oxygen, from the oxides of iron or of manganese. Yet, in the usual method of reducing a metallic oxide, the oxygen is surrounded by a much greater quantity of caloric than is necessary to convert it into gas. Every fixation of a volatile substance is analogous to the present question; and they whose minds have taken alarm from the novelty of the fact, may thus be familiarized with the necessity of admitting it.

Mercury having lost caloric and combined with platina, may be in the same situation as oxygen in many oxides; *viz.* not expellable by mere heat, and perhaps by few affinities.

But, it may be objected, in the instances of iron or manganese, oxygen is combined with a combustible body, and retained in it by a decided and powerful affinity. There is no reason to suppose that such an affinity may not exist among metals. We have been forced to acknowledge it, in a few cases, among the earths; and, from the profound and sagacious researches of Mr. Berthollet, we have learned many new facts, that promise us a rapid increase of knowledge. I shall beg leave to add a few examples, which are taken from that class of bodies to which the subject of the present Paper belongs, and show that the metals obey the general law of mutual attraction.

Metals may combine together by an affinity comparable to that of a metal and oxygen.

(To be concluded in our next.)



## III.

*Investigation of certain Theorems relating to the Figure of the Earth. By JOHN PLAYFAIR, F. R. S. Edin. and Professor of Mathematics in the University of Edinburgh.\**

No complete results have yet been had as to the figure and magnitude of the earth.

## Enumeration.

The results of observed eccentricity agree with the computations deduced from the length of the seconds pendulum.

1. **T**HE observations which have been made to determine the magnitude and figure of the earth, have not hitherto led to results completely satisfactory. They have indeed demonstrated the compression or oblateness of the terrestrial spheroid, but they have left an uncertainty as to the quantity of that compression, extending from about the one hundred and seventieth, to the three hundred and thirtieth part of the radius of the equator. Between these two quantities, the former of which is nearly double of the latter, most of the results are placed, but in such a manner that those best entitled to credit are much nearer to the least extreme than to the greatest. Sir Isaac Newton, as is well known, supposing the earth to be of uniform density, assigned for the compression at the poles  $\frac{1}{230}$ , nearly a mean between the two limits just mentioned; and it is probable, that, if the compression is less than this, it is owing to the increase of the density towards the centre. Boscovich, taking a mean from all the measures of degrees, so as to make the positive and negative errors equal, found the difference of the axes of the meridian  $= \frac{1}{248}$ . By comparing the degrees measured by Father Leisganig in Germany, with eight others that have been measured in different latitudes, La Lande finds  $\frac{1}{311}$ , and, suppressing the degree in Lapland, which appears to err in excess,  $\frac{1}{331}$  for the compression. La Place makes it  $\frac{1}{321}$ ; Sejour  $\frac{1}{367}$ , and, lastly, Carouge and La Lande  $\frac{1}{366}$ .

These results, which reduce the eccentricity of the meridians so much lower than was once supposed, agree well with the observations of the length of the pendulum made in different latitudes. Were the earth a homogeneous body, Sir Isaac Newton demonstrated, that the diminution of gravity under the equator would be  $= \frac{1}{230}$ , expressed by the same fraction with the compression at the poles. M. Clairault made afterwards a very important addition to this theorem: for he shewed, that, if the earth be not homogeneous, but have a

\* Edinburgh Trans. Vol. V.

density that varies with any function of the distance from the centre, the two fractions, expressing the compression at the poles, and the diminution of gravity at the equator, when added together, must be of the same amount as in the homogeneous spheroid, that is, must be  $= \frac{2}{230}$  or  $\frac{1}{115}$ . Now, the second pendulum is concluded, from the best and most recent observations, to be longer at the pole than at the equator by  $\frac{1}{185}$ , and this, taken from  $\frac{1}{115}$ , leaves  $\frac{1}{304}$  for the compression at the poles.

2. But though  $\frac{1}{304}$ , or some fraction not very different from it, should be admitted as the most probable value of the compression, or ellipticity, as it is called, of the terrestrial spheroid, it still remains to be explained, why all the observations, considering the care with which they have been made, do not agree more nearly with this conclusion. Among the causes that may be assigned for this inconsistency; though unavoidable mistakes, and the imperfection of instruments, must come in for a part, there can be little doubt that local irregularities in the direction of gravity have had the greatest share in producing it. Of these irregularities, that which arises from the attraction of mountains has had its existence proved, and its quantity, in one case, ascertained, by the very accurate observations of the present astronomer-royal at Schiehallien in Perthshire. We may trace the operation of this cause in many of the degrees that have been actually measured. Thus, in the degree at Turin, when divided into two parts, and each estimated separately, that which was to the north of the city, and pointed toward Monte Rosa, the second of the Alps in elevation, and the first perhaps in magnitude, was found greater in proportion than that toward the south, the plummet having been attracted by the mountain above mentioned, and the zenith made of consequence to recede toward the south. There are no doubt situations in which the measurement of a small arch might, from a similar cause, give the radius of curvature of the meridian infinite, or even negative.

Why have not the observations of axes agreed better?

Ans.—Principally because the attractions of mountains have caused error in the use of the plumb-line.

But there is another kind of local irregularity in the direction of gravity, that may also have had a great effect in disturbing the accuracy of the measurement of degrees. The irregularity I mean is one arising from the unequal density of the materials under and not far from the surface of the earth; and this cause of error is formidable, not only because it may go to a great extent,

Another more formidable cause of error:



that the density of the strata near the earth's surface must be much greater towards some regions than others.

extent, but because there is not any visible mark by which its existence can always be distinguished. The difference between the primary and secondary strata is probably one of the chief circumstances on which this inequality depends. The primary strata, especially if we include among them the granite, may often have three times the specific gravity of water, whereas the secondary, such as the marly and argillaceous, frequently have not more than twice the specific gravity of that fluid. Suppose, then, that a degree is measured in a country where the strata are all secondary, and happens to terminate near the junction of these with the primitive or denser strata, the line of which junction we shall suppose to lie nearly east and west; the superior attraction of the denser strata must draw the plummet toward them, and make the zenith retire in the opposite direction; thus diminishing the amplitude of the celestial arch, and increasing, of consequence, the geodetical measure assigned to a degree. From suppositions, no way improbable, concerning the density and extent of such masses of strata, I have found, that the errors, thus produced, may easily amount to ten or twelve seconds.

This error may be ten or twelve seconds.

Accurate results are therefore only to be had from large arcs;

3. While we continue to draw our conclusions, about the figure of the earth, from the measurement of single degrees, there appears to be no way of avoiding, or even of diminishing, the effects of these errors. But if the arches measured are large, and consist each of several degrees, though there should be the same error in determining their celestial amplitudes, the effect of that error, with respect to the magnitude and figure of the earth, will become inconsiderable, being spread out over a greater interval; and it is, therefore, by the comparison of two such arches that the most accurate result is likely to be obtained. But, in pursuing this method, since the arches measured cannot be treated as small quantities, or mere fluxions of the earth's circumference, the calculation must be made by rules quite different from those that have been hitherto employed. These new rules are deduced from the following analysis.

and these require new modes of computation.

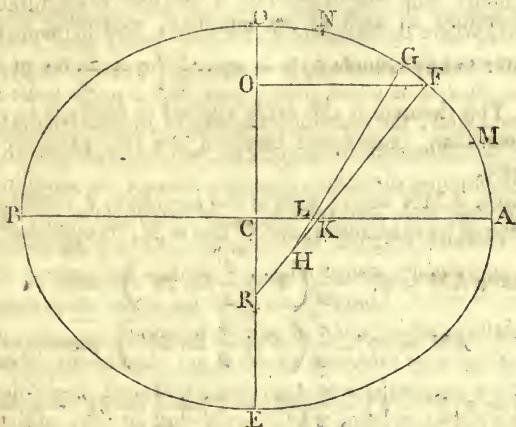
Investigation of formulas for computing the figure of the earth.

4. Let the ellipsis  $ADBE$  represent a meridian passing through the poles  $D$  and  $E$ , and cutting the equator in  $A$  and  $B$ . Let  $C$  be the centre of the earth,  $AC$ , the radius of the equator,  $= a$ , and  $DC$ , half the polar axis,  $= b$ . Let  $FG$  be any very small arch of the meridian, having its



its centre of curvature in H; join HF, HG cutting AC in K and L. Let  $\phi$  be the measure of the latitude of F, or the measure of the angle AKF, expressed, not in degrees and minutes, but in decimals of the radius 1; then the excess of the angle ALG above AKF, that is, the angle LHK or GHF will be  $= \phi$ , and therefore  $FG = \phi \times FG$ . Also, if the elliptic arch  $AF = z$ ,  $FG = z = \phi \times FH$ .

Investigation of  
formulas for  
computing the  
figure of the  
earth.



But FH, or the radius of curvature at F, is =

$$\frac{a^2 b^2}{(a^2 - a^2 \sin^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}} = a^2 b^2 (a^2 - a^2 \sin^2 \phi + b^2 \sin^2 \phi)^{-\frac{3}{2}}, \text{ as is demonstrated in the conic sections. Therefore, if } c \text{ be the compression at the poles, or the excess of } a \text{ above } b, b^2 = a^2 - 2ac + c^2, \text{ or because } c \text{ is small in comparison of } a, \text{ if we reject its powers higher than the first, } b^2 = a^2 - 2ac, \text{ and } FH = a^3 (a - 2c) (a^2 - a^2 \sin^2 \phi + a^2 \sin^2 \phi - 2ac \sin^2 \phi)^{-\frac{3}{2}} = a^3 (a - 2c) (a^2 - 2ac \sin^2 \phi)^{-\frac{3}{2}}.$$

$$\text{But } (a^2 - 2ac \sin^2 \phi)^{-\frac{3}{2}} = a^{-3} \left(1 - \frac{2c}{a} \sin^2 \phi\right)^{-\frac{3}{2}} \\ = a^{-3} \left(1 + \frac{3c}{a} \sin^2 \phi\right) \text{ nearly, rejecting, as before, the terms that involve } c^2, \&c. \text{ Hence } FH = (a - 2c) \left(1 + \frac{3c}{a} \sin^2 \phi\right) = a - 2c + 3c \sin^2 \phi.$$

Now

Investigation of  
formulas for  
computing the  
figure of the  
earth.

Now  $z = \dot{\phi} \times FH$ , therefore  $\dot{z} = \dot{\phi} (a - 2c + 3c \sin^2 \phi)$   
 $= (a - 2c) \dot{\phi} + 3c \dot{\phi} \sin^2 \phi$ . But  $\sin^2 \phi = \frac{1 - \cos 2\phi}{2}$ , there-

fore  $\dot{z} = (a - 2c) \dot{\phi} + \frac{3}{2} c \dot{\phi} - \frac{3c\dot{\phi}}{2} \cos 2\phi$ , and taking the fluent  
 $z = \left(a - \frac{c}{2}\right) \phi - \frac{3c}{4} \sin 2\phi$ . To this value of  $z$  no constant  
 quantity is to be added, because it vanishes when  $z = 0$ .

Therefore an arch of the meridian, extending from the  
 equator to any latitude  $\phi$ , is  $= a\phi - \frac{c}{2} \left(\phi + \frac{3}{2} \sin 2\phi\right)$ .

5. This theorem is also easily applied to measure an arch of  
 the meridian, intercepted between any two parallels of the  
 equator.

Thus, if  $MN$  be any arch of the meridian,  $\phi'$  the latitude of  
 $M$ , one of its extremities, and  $\phi''$  that of  $N$ , its other extremity,

we have  $AM = a\phi' - \frac{c}{2} \left(\phi' + \frac{3}{2} \sin 2\phi'\right)$ , and

$AN = a\phi'' - \frac{c}{2} \left(\phi'' + \frac{3}{2} \sin 2\phi''\right)$ , therefore the arch

$MN = a(\phi'' - \phi') - \frac{c}{2} \left((\phi'' - \phi') + \frac{3}{2} \sin 2\phi'' - \frac{3}{2} \sin 2\phi'\right)$ .

6. If, therefore,  $MN$  be an arch of several degrees of the  
 meridian, the length of which is known by actual measure-  
 ment, and also the latitude of its two extremities  $M$  and  $N$ ,  
 this last formula gives us an equation, in which  $a$  and  $c$  are  
 the only unknown quantities. In the same manner, by the  
 measurement of another arch of the meridian, an equation will  
 be found, in which  $a$  and  $c$  are likewise the only unknown  
 quantities. By a comparison, therefore, of these two equa-  
 tions, the values of  $a$  and  $c$ , that is, of the radius of the  
 equator, and its excess above half the polar axis, may be de-  
 termined.

Thus, if  $l$  be the length of an arch measured,  $m$  the co-effi-  
 cient of  $a$ , and  $n$  of  $c$ , computed by the last formula; and if  $l'$   
 be the length of any other arch,  $m'$  the coefficient of  $a$ , and  $n'$  of  
 $c$ , computed in the same manner, we have  $ma - nc = l$ ,

and  $m'a - n'c = l'$ .

Whence  $a = \frac{n'l - n'l'}{mn' - m'n}$ ;  $c = \frac{m'l - m'l'}{mn' - m'n}$  and  $\frac{c}{a} = \frac{m'l - m'l'}{n'l - n'l'}$ . It

may

may be useful, in the numerical calculation, to observe also that

$$c = \frac{ma - l}{n}.$$

7. The arch of the meridian, which was measured in Peru, compared with that measured in France, will afford an example of the application of these formulas.

Application of the formulas to the arc measured in Peru and that in France.

The amplitude of the arch measured in Peru was  $3^{\circ} 7' 1''$ , and its length 176940 toises. To reduce this to the level of the sea, above which it was elevated 1226 toises, 66 toises must be subtracted, and again 12 toises added to adapt it to the mean temperature of the atmosphere. Thus corrected it is 176886 toises. The arch measured begun  $36''$  north of the equator, and extended to the parallel of  $3^{\circ} 6' 25''$  south; we shall suppose it to have begun under the equator, and to have extended to the parallel of  $3^{\circ} 7' 1''$ , a supposition which can produce no sensible error, and will somewhat simplify the calculation. Thus  $\phi$ , in the preceding formula, is an arch of  $3^{\circ} 7' 1''$  expressed in decimals of the radius 1, and so we have  $m = .0544009$ ,  $n = .1086408$ , and  $l = 176886$ .

Again, the amplitude of the whole arch measured in France from Dunkirk to Perpignan is  $8^{\circ} 20' 2''\frac{1}{2}$  and its length 475496 toises. The northern extremity of this arch is in latitude  $51^{\circ} 2' 1''$ , and the southern in  $42^{\circ} 41' 58''\frac{1}{2}$ . Hence  $\phi'' = .8907045$ , and  $\phi' = .7452459$ , and therefore  $m' = .1454586$ ,  $n' = .0585735$ ,  $l = 475496$ .

Therefore,  $a = \frac{n'l - nl'}{mn' - m'n} = 3273325$  toises;

$$c = \frac{m'l - ml'}{mn' - m'n} = 10917 \text{ toises,}$$

$$\text{and } \frac{c}{a} = \frac{1}{300} \text{ nearly.}$$

Wherefore also the longer axis of the meridian is to its conjugate, or  $a$  is to  $b$  as 300 to 299.

This proportion agrees well with that which was already pointed out as the most probable result, from the comparison of single degrees, and from observations of the pendulum. As these conclusions are obtained by different methods, they tend greatly to confirm one another.

8. From this too it seems highly probable, that the uncertainty which yet remains with respect to the true figure of the earth will be well determined

Probability that the figure of the earth will be well determined



by measurement  
of some other  
considerable  
arcs.

earth will be entirely removed by the measurement of some other considerable arches of the meridian. Such an arch will be furnished by the survey of Great Britain begun by General Roy, and still continued in a style of accuracy so much superior to any other system of geometrical operations that has ever yet been executed. In drawing the conclusions from observations made with such exactness, it may be necessary to employ a more accurate approximation than has been done in the preceding formula, by retaining the second power of  $c$ . The equations to be resolved will thus become of the second order, but as the unknown quantities can be nearly found by the solution of a simple equation, the farther approximation to their true values will be accompanied with no difficulty.

Farther approxi-  
mation to be  
then employed.

9. Concerning this farther approximation it may be useful however to remark, that if  $c^2$  be retained, its coefficient in the

formula of § 4 will be  $\frac{1}{16a} \left( \phi + \frac{15}{4} \sin 4\phi \right)$ ;

and therefore in the formula of § 5 it will be

$$\frac{1}{16a} \left( \phi'' - \phi' + \frac{15}{4} (\sin 4\phi'' - \sin 4\phi') \right).$$

If then the quantity

$$\frac{1}{16a} \left( \phi'' - \phi' + \frac{15}{4} (\sin 4\phi'' - \sin 4\phi') \right),$$

computed for any arch of the meridian, be put  $= g$ , and the same, computed for any other arch, be  $= g'$ , the equations of § 6 will become,

$$ma - nc + \frac{gc^2}{a} = l, \text{ and}$$

$$m'a - n'c + \frac{g'c^2}{a} = l'.$$

10. Here if we put  $d$  for the value of  $a$ , as given by the formula  $\frac{n'l - nl'}{mn' - m'n}$ ; and  $h$  for the value of  $c$ , as given by the for-

mula  $\frac{m'l - ml'}{mn' - m'n}$ , also  $v$  for the correction to be made on  $d$ , and  $u$  for the correction to be made on  $h$ , so that  $a = d + v$ , and  $c = h + u$ , by substituting these values of  $a$  and  $c$  in the two last equations we have  $mv - nu + \frac{g(h+u)^2}{d+v} = 0$ , and

$$m'v - n'u + \frac{g'(h+u)^2}{d+v} = 0.$$

Hence

Hence, rejecting all the terms that involve  $v^2$ ,  $u^2$ , or  $uv$ , we have  $dmv - dnu + gh^2 + 2ghv = 0$ , and  $dm'v - dn'u + g'h^2 + 2g'hv = 0$ . Farther approximation to be then employed.

Therefore,  $v = \frac{(ng' - n'g)h^2}{(n'm - nm')d + (gn' - gn)2h}$  also

$u = \frac{g'h^2(dm + 2gh) - gh^2(dn' + 2g'h)}{dn'(dm + 2gh) - dn(dm' + 2g'h)}$

And again, by rejecting those terms that are small in comparison of the rest,  $v = \frac{h^2(ng' - n'g)}{d(n'm - nm')}$ , and

$u = \frac{h^2(g'm - gm)}{d(n'm - nm')}$

Thus  $v$  and  $u$  are found, and of consequence  $d+v$  and  $h+u$ , that is  $a$  and  $c$ , without neglecting any terms that are not of an order less than  $\frac{c^2}{a}$ ; and when it is considered that

$\frac{c^2}{a}$  is less than  $\frac{1}{22500}$ , it will readily be allowed that it is quite

unnecessary to carry the approximation farther.

11. The same thing that renders the comparison of large arches of the meridian useful for lessening the effect of errors arising from irregularities in the direction of gravity, makes it serve to diminish the effect of all the errors of the astronomical observations at the extremities of the arches, from whatever cause they arise. They are all diffused over a greater interval, and have an effect proportionally less in diminishing the accuracy of the last conclusion. The errors of observation are less the larger the arcs.

12. The measurement therefore of large arches of the meridian, especially if performed in distant countries, is likely to furnish the best data for ascertaining the true figure of the earth; and on this account extensive and accurate surveys, such as that above mentioned, are no less interesting to science, in general, than conducive to national utility. The survey of this island, when completed, will furnish an arch of the meridian, beginning at the same parallel where that measured in France terminates, and nearly of the same extent, so that the length of an arch of more than  $16^\circ$ , or almost a twentieth of the earth's circumference, will become known. The different portions of this arch compared with one another, or with the arch

arch measured in Peru, will afford a variety of *data* for determining the true figure of the earth.

Observations of perpendiculars to the meridian, and of parallels of latitude.

But surveys of the kind now referred to, afford likewise other materials from which the solution of this great geographical problem may be deduced. These are chiefly of two sorts, viz. the magnitude of arches, either of the curves perpendicular to the meridian, or of the circles parallel to the equator. Examples of the first of these have been given by General Roy and Mr. Dalby; the observations which follow are directed toward both.

Large perpendiculars are of very difficult measurement;

and small ones erroneous from partial gravitation.

13. With respect to the measurement of arches perpendicular to the meridian, it may be observed, that the directions of gravity at different points of such arches do not intersect one another at all, unless the distances of those points from the said meridian be very small. On this account the measurement of a large arch perpendicular to the meridian would involve in it considerable difficulty; to avoid which it is necessary that the arch measured be but small, or one that does not greatly exceed a single degree. Such measurements are of course obnoxious to all the errors that arise from the deflection of the plumb-line, and cannot therefore furnish *data* for determining the figure of the earth, equally valuable with those which may be derived from large arches of the meridian. The method of determining the figure of the earth, from degrees of the perpendicular to the meridian, is not however without its advantages, and in certain circumstances is preferable to any other that proceeds by the measurement of arches equally small. This method is twofold; as a degree of the meridian may be compared with a degree of the perpendicular to it in the same latitude; or two degrees perpendicular to the meridian, in different latitudes, may be compared with one another. The advantages peculiar to each will appear from the following investigation.

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpendicular.

14. Let it be required to find the axes of an elliptical spheroid, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the same latitude.

Let the ellipsis *ADBE* (Fig. on page 105) represent a meridian, of which a degree is measured at *F*. Let the perpendicular



dicular to the meridian in F meet the less axis DE in R. Then R will be the centre of curvature of the circle cutting the meridian at right angles in F; for at any point in that circle indefinitely near to F, the direction of the plumb-line, or of gravity, as it always passes through the axis DE, will cut DE in R; it will therefore also intersect FR in R, so that R is the centre, and FR the radius, of curvature of the perpendicular to the meridian. Let H be the centre of curvature of the meridian itself at F: draw FO perpendicular to DE, and let the latitude of F, or the angle OFR =  $\phi$ . Also let AC =  $a$ , CD =  $b$ , and  $a - b = c$ , as before.

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpend.

Then from the nature of the ellipsis, FO =

$$\frac{a^2 \cos \phi}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}}, \text{ and because } \sin FRO : 1 :: FO : FR,$$

$$\text{that is, } \cos \phi : 1 :: FO : FR, \text{ FR} = \frac{a^2}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}};$$

and this, therefore, is the radius of curvature of the section of the spheroid perpendicular to the meridian at F. But the radius of curvature of the meridian at F, that is FH =

$$\frac{a^2 b^2}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}}, \text{ therefore}$$

$$FR : FH :: \frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{1}{2}}} : \frac{a^2 b^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{3}{2}}},$$

and dividing both by  $\frac{a^2}{(a^2 \cos^2 \phi + b^2 \sin^2 \phi)^{\frac{1}{2}}}$ , we have

$$FR : FH :: a^2 \cos^2 \phi + b^2 \sin^2 \phi : b^2,$$

15. If then D be the length of a degree of the meridian at F and D' the length of a degree of the circle at right angles to it,

$$D' : D :: a^2 \cos^2 \phi + b^2 \sin^2 \phi : b^2, \text{ and } \frac{D'}{D} =$$

$$\frac{a^2 \cos^2 \phi + b^2 \sin^2 \phi}{b^2} = \frac{a^2}{b^2} \cos^2 \phi + \sin^2 \phi. \text{ Hence}$$

$$\frac{D'}{D} - \sin^2 \phi = \frac{a^2}{b^2} \cos^2 \phi \text{ and } \frac{a}{b} = \frac{\sqrt{\frac{D'}{D} - \sin^2 \phi}}{\cos \phi}.$$

This last formula, therefore, gives the ratio of  $a$  to  $b$  when D, D' and  $\phi$  are known.

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpend.

16. To find  $a$  and  $b$  themselves, if  $m = 57.2957$ , &c. or the number of degrees in the radius, so that  $mD' = FR =$

$$\frac{a^2}{(a^2 \operatorname{cof} \varphi^2 + b^2 \sin \varphi^2)^{\frac{1}{2}}}, \text{ and since it has been already shown that } \frac{a^2}{b^2} = \frac{\frac{D'}{D} - \sin \varphi^2}{\operatorname{cof} \varphi^2}, \text{ or } b^2 = \frac{a^2 \operatorname{cof} \varphi^2}{\frac{D'}{D} - \sin \varphi^2}, \text{ there-}$$

$$\text{fore } mD' = \frac{a^2}{\left( a^2 \operatorname{cof} \varphi^2 + \frac{a^2 \operatorname{cof} \varphi^2}{\frac{D'}{D} - \sin \varphi^2} \times \sin \varphi^2 \right)^{\frac{1}{2}}} =$$

$$\frac{a}{\operatorname{cof} \varphi \left( 1 + \frac{\sin \varphi^2}{\frac{D'}{D} - \sin \varphi^2} \right)^{\frac{1}{2}}} \text{ and } a =$$

$$mD' \operatorname{cof} \varphi \sqrt{1 + \frac{\sin \varphi^2}{\frac{D'}{D} - \sin \varphi^2}}.$$

$$\text{Now, } 1 + \frac{\sin \varphi^2}{\frac{D'}{D} - \sin \varphi^2} = \frac{\frac{D'}{D}}{\frac{D'}{D} - \sin \varphi^2} = \frac{1}{1 - \frac{D}{D'} \sin \varphi^2},$$

$$\text{therefore } a = \frac{mD' \operatorname{cof} \varphi}{\sqrt{1 - \frac{D}{D'} \sin \varphi^2}}.$$

17. This value of  $a$  is very convenient for logarithmical calculation; for if  $\sin \varphi \sqrt{\frac{D}{D'}}$  be computed, it will always be less than 1, because  $D'$  is greater than  $D$ , and therefore may be taken for the sine of an arch  $\psi$ , of which arch  $\sqrt{1 - \frac{D}{D'} \sin \varphi^2}$  will of course be the cosine, so that  $a = \frac{mD' \operatorname{cof} \varphi}{\operatorname{cof} \psi}$ .

The same method may be used for finding  $\frac{a}{b}$  from the formula in § 15.

In

In the same manner that  $a$  has been found, we will obtain  $b$  =  $\frac{mD' \cos \phi^2}{\left(1 - \frac{D}{D'} \sin \phi^2\right) \sqrt{\frac{D'}{D}}}$  To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpend.

If we examine these formulas in the extreme cases, viz. when  $\phi = 90^\circ$ , and when  $\phi = 0$ , we shall have in the former case  $a = \frac{0}{0}$ , because  $\cos \phi = 0$ , and also  $D' = D$ , so

that  $1 - \frac{D}{D'} \sin^2 \phi = 0$ . Here therefore  $a$  is indefinite, and may be of any magnitude whatever; and it is evident that this is the result which the formula ought to give: because at the pole, or when  $\phi = 90^\circ$ , the perpendicular to the meridian is itself a meridian, and therefore the measurement of the two degrees,  $D$  and  $D'$ , is but the same with the measurement of one degree.

When  $\phi = 0$ , that is at the equator, the circle perpendicular to the meridian is the equator itself, and we have then  $a = mD'$ ,  $a$  being determined in this case by the degree of the equator alone. Here also we have  $\frac{a}{b} = \sqrt{\frac{D'}{D}}$ , which is known to be true.

13. The preceding formulae may be rendered more simple, if we aim only at an approximation, which indeed is all that is necessary in this inquiry. Since  $c$  denotes the compression, or since  $a - c = b$ , and therefore  $a^2 - 2ac = b^2$  nearly, consequently the radius of curvature of the meridian at  $F$ , that is

$$mD = \frac{a^2 (a^2 - 2ac)}{(a^2 - 2ac \sin \phi^2)^{\frac{3}{2}}} = \frac{a^3 (a - 2c)}{a^3 \left(1 - \frac{2c}{a} \sin \phi^2\right)^{\frac{3}{2}}} =$$

$$(a - 2c) \left(1 - \frac{3c}{a} \sin \phi^2\right), \text{ or } mD = a - 2c + 3c \sin \phi^2. \text{ In}$$

the same manner  $mD' = a + c \sin \phi^2$ . From these equations we obtain, rejecting always the higher powers of  $c$ ,

$$c = \frac{m(D' - D)}{2 \cos \phi^2}, \quad a = mD' \frac{m(D' - D) \sin \phi^2}{2 \cos \phi^2}; \text{ and}$$

$$\frac{c}{a} = \frac{D' - D}{2D' \cos \phi^2}.$$

These formulas may be transformed into others a little more convenient for computation, by putting  $\sec \phi^2$  instead of



$\frac{1}{\cos \phi^2}$ , and  $\tan \phi^2$  instead of  $\frac{\sin \phi^2}{\cos \phi^2}$ ; we have then,

$$e = \frac{m}{2} (D' - D) \sec \phi^2,$$

$$a = mD' - \frac{m}{2} (D' - D) \tan \phi^2, \text{ and}$$

$$\frac{e}{a} = \frac{(D' - D)}{2D'} \sec \phi^2.$$

Example of the compression deduced from the meridian and its perpendicular.

19. We may apply these formulas to the computation of  $\frac{e}{a}$ , &c. from the degrees of the meridian and perpendicular, measured in the south of England. We find, in one example; (*Phil. Trans.* 1795, p. 537), that  $D = 60851$  fathoms,  $D' = 61182$ , the latitude, or  $\phi$  being  $= 50^\circ 41'$ . From this  $\frac{e}{a} =$

$$\frac{D' - D}{2D' \cos^2 \phi} = \frac{331}{2 \times 61182 \times (\cos 50^\circ 41')^2} = \frac{1}{148.4}, \text{ which}$$

is nearly the same result with that deduced in the passage just referred to. Indeed the solution of this problem, contained in the *Trigonometrical Survey*, is quite unexceptionable; and the theorems here offered are not given as containing a more accurate solution, but one that is in some respects more simple.

Whether the magnitude of this result may prove an irregular figure.

The above compression, if the remarks already made be well founded, is much too great, being more than double of what was obtained from comparing the whole arch of the meridian measured in France with the whole of that measured in Peru. At the same time it is right to observe, that all the other comparisons of the degrees of the meridian, with those of the curve perpendicular to it, made from the observations in the south of England, agree nearly in giving the same oblateness to the terrestrial spheroid. For this circumstance, it is certainly not easy to account; the unparalleled accuracy with which the whole of the measurement has been conducted, makes it in the highest degree improbable that it arises from any error; and even if errors were to be admitted, it is not likely that they should all fall on the same side. The authors of the *Trigonometrical Survey* seem willing, therefore, to give up the elliptic figure of the earth, (*Ibid.* p. 527); but before we abandon that very natural and simple hypothesis, it may perhaps be worth while to attend to the following considerations.

20. In the part of England, where the measures we are now treating of have been taken, the strata are of chalk; and though of great extent, are bordered, on all the sides that we have access to examine, by strata much denser and more compact. Toward the west the chalk is succeeded by limestone; and that limestone by the primitive schistus and granite of the west of Devonshire and of Cornwall. On the east we may suppose that something of the same kind takes place, though the sea prevents us from observing it, as the chalky and argillaceous beds extend in this direction to the coast, and probably to some distance beyond it. Now the meridian of Greenwich may be considered as dividing the tract of country, occupied by these lighter strata, into two parts, in such a manner, that the plummet being carried to a distance from it, either east or west, approaches to the denser strata, and is of course attracted by them, so that the zenith is forced back, as it were, to the meridian of Greenwich, and does not recede from it, in the heavens, at so great a rate as the plummet itself does, on the earth. Hence the longitudes from this meridian, estimated by the arches in the heavens, intercepted between the zenith and the said meridian, will appear less than they ought to do; and too much space on the surface of the earth will of consequence be assigned as the measure of a degree. In this way  $D'$  is made too great; and we may suppose the circumstances such that  $D$ , on going north or south, is not enlarged in the

Observations to show that the irregularity probably arises from local circumstances.

The meridian of Greenwich appears to divide a chalk country, bearing on each side upon denser strata of limestone, schistus, and granite, which, by attracting the plumb-line either way outwards, must require a longer measure of easting or westing to correspond with an apparent celestial arc.

same proportion; hence  $\frac{D' - D}{D'}$  will be augmented, and of

course  $\frac{c}{a}$  will be represented as too great. This explanation

may perhaps appear very hypothetical, and it is certainly proposed merely as a hypothesis. It is a hypothesis, too, that lays claim only to a temporary indulgence, as it is proposed at the very moment when it may be brought to the trial, and when, by a further continuation of the survey toward the north, it will probably be determined how far the distribution of the strata of this country affects the direction of gravity. It will indeed be curious to remark what irregularities take place on advancing into the denser strata of the north. The limestone and sandstone strata of the middle part of the island will succeed to the chalk of the south, the primitive and denser strata still occupying



occupying the west, at least at intervals, as in Wales, Cumberland, and Galloway. Further to the north, that is, beyond the Tay, the strata became entirely primitive, most of them of the densest kind, and in the interior of the island, with a very few exceptions, continue the same to its most northern extremity. In the survey of Britain, therefore, several situations must occur where the plummet, passing from lighter to denser strata, ought to give indications of some irregularities in the direction of the gravitating force. It will be seen hereafter how far these conjectures are verified by experience.

(To be continued.)

#### IV.

*On the Determination of the Length of the Solar Year.* By Mr.  
R. WINTER.

*Red Cross Wharf, London-Bridge,  
13th Jan. 1804.*

To Mr. NICHOLSON,

SIR,

Determination  
of the length of  
the solar and  
sidereal years.

SHOULD you consider the following communication as deserving a place in your valuable Journal, by inserting it you will oblige

Your's, &c.

R. WINTER.

To determine the precise length of the Solar Year is an object of primary utility in astronomy, as being the basis for ascertaining the periods of all the moving bodies in the universe.

Various and accurate methods have been given for determining this period; but as the following unites simplicity, to the advanced state of science, these characters may render it worthy of your consideration.

Given, the time and place of the sun in the ecliptic, and the time of its returning to the same place; the obliquity of the ecliptic for the present time, together with the decrease thereof, per year, to find the length of the solar year.

According to Ptolemy, the place of the sun was Sagittarius  $21^{\circ} 22' 50''$  anno. 130, Dec. 13th.  $11^{\text{h}} 38^{\text{m}} 40^{\text{s}}$  (reduced to the

the



the meridian of London,) its return to that place in 1804 according to Mayers tables improved, is Dec. 13th. 0<sup>h</sup>. 19<sup>m</sup>. 36<sup>s</sup> the interval, (allowing for biffextiles, &c.) is 611416<sup>D</sup>. 12<sup>h</sup>. 40<sup>m</sup>. 56<sup>s</sup> which divided by 1674, the number of years, gives 365<sup>D</sup>. 5<sup>h</sup>. 49<sup>m</sup>. 42<sup>s</sup>. Now the obliquity of the ecliptic, as observed by Cassini in 1655, and Flamsteed in 1689, being compared with that observed by Hornsby in 1772, Maskelyne in 1769, Bradley in 1750, and Mayer in 1756, and a mean of their observations taken, will give for the decrease of the obliquity of the ecliptic 59" per century, or 35'."4 per year. According to these calculations the obliquity of the ecliptic for 1804 will be about 23° 27' 49"; then as the sun moves over this space in one quarter of a year, say as 23° 27' 49" is to  $\frac{1}{4}$  of the given year, so is the decrease of the obliquity of the ecliptic per year 35'."4 unto 55", which taken from 365<sup>D</sup>. 5<sup>h</sup>. 49<sup>m</sup>. 42<sup>s</sup> gives for the length of the solar year 365<sup>D</sup>. 5<sup>h</sup>. 48<sup>m</sup>. 47<sup>s</sup> and the time the sun moves over the proceffion of the equinoxes is 20 minutes 27 seconds, which being added to the solar year will give 365<sup>D</sup>. 6<sup>h</sup>. 9<sup>m</sup>. 14<sup>s</sup> for the length of the sidereal year.

But by assuming  $\frac{1}{4}$  of the year thus found, and working as before, it will give the length of the year more accurately.

## V.

*Extracts from a Letter of R. Chenevix, Esq. F. R. S. from Dresden, to Charles Hatchett, Esq. F. R. S. containing Information respecting the new Metal contained in Crude Platina; Beet Sugar; the non-existence of Agassile as a peculiar Earth; and a Description of a new Furnace for Chemical Operations. Communicated by C. HATCHETT, Esq.*

I SUPPOSE you have heard of the new metal contained in raw platina. Here is what Berthollet has just written to me upon the subject:

New metal in  
crude platina.

"Des Costils dissolves raw platina in nitro-muriatic acid, and precipitates it by muriate of ammonia at several times; the first portions are yellow, the last redder. He reduces the red precipitate, and obtains an alloy. He exposes this alloy to a current of oxygen, and a blue oxide is volatilized; pure platina remains behind. The blue sublimate is the oxide of his new

new metal; on the same day Fourcroy and Vauquelin read their paper at the Institute, and mentioned similar experiments."

Palladium asserted to be an alloy of the new metal with mercury.

Van Mons writes me word that at Paris they say that it is not platina, but this new metal, which with mercury composes palladium; but that has very little to say to the purpose, for the singular thing in palladium is, not what regards platina, but what regards mercury, and the fusibility of the combination. Besides there must have been platina in my palladium, as I took the metal reduced from the red and yellow salts indiscriminately.

Manufacture of beet sugar by Lampadius.

Lampadius tells me that he has made several hundred weight of sugar from the red beet; but the manufactory he established has failed. It cost him within two gros per lb. as much as the common sugar of equal quality, but then he had a residuum fit for fermentation, which was clear profit.

Agustite is merely phosphate of lime.

I had made some progress in the analysis of the Saxon beryl, when I received a letter from Berthollet, in which he told me that Vauquelin had found the agustite to be nothing more than phosphate of lime. I continued my experiments however, and obtained the following results. Having boiled muriatic acid upon the pulverized mass which contains the Saxon beryl,

Muriatic solution of Saxon beryl precip. by amm. gave phosphoric acid. This was decomposed by sulph. acid; and the purified phosphoric acid reduced to actual phosphorus.

I obtained a solution of the pretended agustite. I precipitated by ammonia, and, upon examining the precipitate, found it to be phosphate of lime. To have no doubt as to the phosphoric acid, I treated 1500 grains of this precipitate with half its weight of sulphuric acid, washed the mass, and evaporated to dryness. I re-dissolved as much as was soluble in distilled water, and saturated the liquor by ammonia. By evaporating once more, I obtained phosphate of ammonia, which I decomposed at a strong red heat, and obtained phosphoric acid, which gave no trace of sulphuric or any other acid, or of any earth, alkali, or metal. I mixed this with pounded charcoal, and by distillation in a strong fire obtained a considerable quantity of phosphorus. Thus then the simple earth of Mons. Tromsdorf is phosphate of lime.

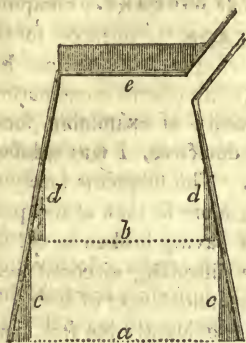


I have constructed a wind furnace here, which is in some respects to be preferred to the usual form. The sides, instead of being perpendicular, are inverted, so that the hollow space is pyramidal. At the bottom the opening is thirteen inches square,

and at the top but eight. The perpendicular height is 17 inches.

This form appears to me to unite the following advantages. 1st. A great surface is exposed to the air; which, having an easy entrance, rushes through the fuel with great rapidity. 2d. The inclined sides act in some measure as reverberating surfaces: And 3d. The fuel falls of itself, and is always in close contact with the crucible, placed near

Chemical furnace. Frustum of a cone, narrowest above. Its great advantage is that the fuel descends without the help of stirring.



the grate. This I believe to be the principal advantage. The late Dr. Kennedy of Edinburgh, whose opinion on this subject claims the greatest weight, found that the strongest heat in our common wind furnaces was within two or three inches of the grate. That therefore is the most advantageous position for the crucible, and still more so when we can keep it surrounded with fuel. It is inconvenient and dangerous for the crucible to stir the fire often to make the fuel fall, and the pyramidal form renders this unnecessary. It is also more easy to avoid a sudden bend in the chimney by the upper part of the furnace advancing as in this construction. *a* is a grate; *c* and *c* are two bricks, which I can let in at pleasure to diminish the capacity; *b* is another grate, which I can place upon the bricks *c* and *c* for smaller purposes; *d* and *d* are bricks which I can place upon the grate *b* to diminish the upper capacity, so that in fact I have four different sizes in the same furnace. I have had some very strong fires in using the whole capacity from the grate *a* to the top *e* without the bricks *c* and *c*; but I am at a loss for our good English coaks. The bricks have all been ground down to the slope of the furnace and fit in with tolerable accuracy. They are totally independent of the pyramidal form, &c. of the furnace.



## VI.

*Account of an Experiment to imitate the Damascus Sword Blades,  
In a Letter from Mr. JAMES STODART.*

To Mr. NICHOLSON,

DEAR SIR,

On sword blades  
in general.  
Quality too  
often defective.

Experiment.  
Steel and iron  
bars were welded  
together.

Forged out,

Twisted, flat-  
tened, again  
welded and ham-  
mered flat.

An edge of steel  
was welded into  
a back of the  
compound,

HAVING lately had an opportunity of examining some sword blades, which appeared to be defective, I was induced to make the following experiment. The subject is surely of some importance, and perhaps never more so than at the present moment. We hear of swords having broken in battle, and we can hardly imagine a more distressing circumstance. Those which I have seen are certainly in no danger of failing in that way, for on the contrary they are evidently too soft, and consequently cannot form a good cutting edge. I am not acquainted with the process used in making sword blades, but am inclined to suspect that the price allowed is not equal to the labour necessary to form a good instrument. The following method, which I believe to be nearly the same as that practised at Damascus, but which I suspect would be too difficult and expensive for general application, may perhaps lead to some more simple method of accomplishing the desired purpose. I took six small bars of good malleable iron, and the same number of sheer steel, and laid them one on another alternately, as if forming a galvanic pile; I then with the assistance of an expert workman, committed them to a clean forge fire, and with care we succeeded in welding them into a solid lump. This was forged into a stout flat plate, which being heated to whiteness, was by means of strong tongs twisted spirally until it formed a cylindrical tube. In this twisted state it was heated, hammered flat, and again welded, and after being forged into a convenient form and substance, was doubled throughout its whole length, somewhat in the manner of the back of a saw. A slip of good steel was inserted, and another welding heat taken, which consolidated the whole mass. I need not say this slip of steel was intended for our edge. The remaining part of the process was simple; it consisted only in forging it into the shape of the blade we wanted; which on examination proved perfectly sound in every part. Being eager to witness some proofs of excellence and beauty which my expectation

had

had anticipated, I too hastily and without due consideration proceeded to harden it by heating and quenching in water; and had the misfortune to see it cracking in seven or eight different places. I have no doubt this was occasioned by the unequal expansion and subsequent contraction of the different parts of the mass. In my next trial I shall guard against this accident. Enough however remained sound to prove it both good and beautiful; the edge bears the severest trials at the same time that the whole blade has sufficient tenacity. I have polished a part of it, and by applying a weak acid, produced an appearance, which though by no means equal to the beauty of what is called the Damascus water, leaves me little reason to doubt of accomplishing that appearance in my next trial. My intention is to multiply my pieces of metal, to repeat the process of twisting, and certainly not again to quench in water. I shall take the liberty to transmit to you an exact account of my next experiment, and if successful, to accompany it with a sample of the metal formed into a blade of some kind or another. I am with much respect,

It cracked in hardening.

—but promises well.

The Damascus water.

DEAR SIR,

Your obedient servant,

J. STODART.

*Strand, Jan. 19th, 1804.*

P. S. Why is the appearance produced on Damascus steel by the application of an acid called the water? Is it not different degrees of oxidation? \* and what is the acid best fitted to produce this appearance. I had a paper given me some ten years ago on this subject, by a gentleman whose name I do not know. Unfortunately I have mislaid it.

Inquiries concerning the Damascus water upon steel.

In addition to what you have published on the subject in your valuable Journal, pray furnish us with any other facts that may have come to your knowledge since that period. The subject appears to me to be worthy of philosophical research, and perhaps of national encouragement.

\* I have always supposed steel to be less readily soluble than pure iron; and that the carbon which is seen on the face of the former during the process of damasking, defends it from the acid, while the fibres of iron are etched by corrosion so as to exhibit the peculiar waving lines of this operation. N.

*Letter*



## VII.

*Letter from a Correspondent on the Effects of Thunder on fermenting Liquids; the Chemical Action of Sound, and Tremulous Motions, with other Observations.*

Newcastle, Dec. 16, 1803.

To Mr. NICHOLSON.

SIR,

CONVERSATION is certainly one of the most desirable means for the increase and dissemination of knowledge, whatever description it may be of, and if the following remarks, arising from an accidental conversation with a scientific friend, be not altogether unworthy of your notice, you may make what use of them you think proper; and if thought worthy of being presented to the public, you are at liberty to make any necessary alteration in the style that will render them more worthy of the honour I solicit.

Changes produced in beer and in cream by thunder.

It has been observed, so often indeed, that it has almost become a popular remark, that the noise of thunder produces a surprising change on beer, and on cream: the first becoming sour, and the last rancid, when they are agitated by this extraordinary sound. It has also been observed, I believe, that the effects of thunder on malt liquors may be prevented by laying a cord over the cask, at each end of which, a stone, or other hard and ponderous substance is suspended and kept in contact with the outside of the vessel containing the liquid liable to be affected by this kind of noise.

Supposed preventative remedy.

The effect supposed to arise from tremulous action.

Now we may be able to form some judgment of the manner in which these liquids are affected, by attending to the tremulous motion produced on those glasses called singing and musical glasses, when the surface of the first is agitated by the breath; and the liquid in the last by the vibration produced by the finger drawn along the rim of the glass, which varies in its tone in proportion to the quantity of liquid it contains. In the course of the tone or sound, if the glass be pressed with the finger, the sound either ceases or is considerably diminished, and upon application of the finger at the commencement of the sound it will be evidently felt that both the glass and the liquid are in a state of agitation, and that this agitation ceases or stops when the agitated body is touched by another in a quiescent state.

This



This tremor, so communicated, may in my opinion be the commencement of that insensible or rather imperceptible agitation necessary to the production of the different states of fermentation, which when once began, may be continued, provided the circumstances of temperature, &c. be equally favourable as at first. Now we perceive that this motion is produced by sound; \* and a sonorous body loses this vibration on being touched, and the sound itself of course ceases. In like manner the intestine motion arising from the vibration occasioned by the noise of thunder, being prevented from taking place by the contact of the suspended stones with the sides of the cask, the ensuing fermentation cannot commence, and the beer is preserved from running into the acetous fermentation.

Which may produce the commencement of the fermentative agitation.

This insensible motion is also necessary to assist the absorption of oxygen, without which neither beer can grow sour, nor can cream become rancid; for violent agitation will not produce the like effect even though accompanied or assisted with the necessary increase of temperature.

It was also observed by the gentleman with whom I was conversing on this subject, that butter by being over much washed or wrought in water loses considerably of its yellow colour, and acquires in a great measure the paleness and consistence of tallow. It might be worth while to make some experiments in this way, how far animal oils or fat are capable of decomposing atmospheric air or water. The circumstances necessary to determine the mutual change on each, are agitation, and a frequent change of surface. The absorption of oxygen would take place in both cases, the residuum of atmospheric air being azotic gas, and that of water hydrogen gas. I had found by experiments, and that some time before I saw the circumstance publicly noticed, I may say, even before the new theory of chemistry was promulgated, that the union between mercury and the fat with which it was combined in the composition of the ointment, became much more intimate and perfect on being kept for some time, and that the rancidity, which I thought arose from the developement of some peculiar acid, was prevented from becoming sensible by that acid being absorbed by the metal. From this circumstance, and from this supposition, I was led contrary to the directions and commands

Decomposition of water or air by oils.

Rancid oils combine better with mercury than fresh.

\* I mean among other causes producing it.

of the Royal College of Physicians, to use a little rancid fat or oil, in order to facilitate my labour and favour my indolence, and I was agreeably surpris'd at my success. The College order the purest and sweetest fat to be used; nevertheless the utmost diligence and care in rubbing the mercury with it does not produce such an accurate combination at first as is accomplished, after being set by for a week or two, and this may be made evident by comparing two specimens of ointment made with equal care and diligence, the one being quite recently finished, and the other having afterwards been kept undisturbed for two or three weeks.—I merely offer these hints for the consideration of wiser heads than mine, and should they with this view be honoured with a place in your valuable repository, your indulgence will be most respectfully acknowledged by

Sir,

Yours, &c.

NORTHUMBRIENSIS.

### VIII.

*Improvement of the Electrical Machine, chiefly with respect to the Rubbers; by Mr. WOLFF, of Hanover\*.*

Description of  
the machine.

The plate.

MY electrical apparatus is a plate machine, constructed on the plan of that described by Van-Marum†, with a few slight alterations, which are principally in the rubbers. The plate is eighteen inches in diameter, and the piece of wood, which receives the screw that fastens the plate to the axis, is an inch and half in diameter. The rubbers (at the extremities of which, toward the axis, are sticks of black sealing-wax, rounded at each end, and overhanging or projecting on three sides) are in contact with the glass for the length of  $5\frac{3}{4}$  inches. Consequently the distance from the circular or central piece of wood to the rubbers is  $2\frac{1}{8}$  inches, to which extent the plate

\* From Gilbert's *Annalen der Physik*, 1802, No. 13. p. 601.

† *Observations sur la Physique*, Vol. 38. p. 437. or *Philosophical Journal*, 4to. Series, No. 2.



is coated on both sides with a sufficient thickness of varnish \*. The bow of the conductor, which consists of a ball six inches in diameter, to which is added a moveable tube with several fitting pieces that terminate in balls of different sizes, had at first, at each of its extremities, as in Van-Marum's machine, a brass cylinder three inches long and one inch thick, with an hemisphere at each end. But as conductors of this construction are liable to discharge themselves, on account of the small diameter of their hemispheres, even before they have received their full charge, I have preferred putting one brass ball three inches in diameter on the upper end of the superior conductor; and another on the lower end of the inferior conductor. These balls likewise serve the purpose of collecting the fluid excited at the extreme parts; for the balls, both above and below, project considerably beyond the thickness of the plate, and prevent the escape of the fluid as much as is possible, particularly in a machine less high in proportion than that of Van-Marum. The conductor is only  $4\frac{1}{2}$  inches distant from the table, consequently only half the distance of Van-Marum's; yet the fluid does not escape, till the conductor is overcharged. The escape of the fluid is prevented likewise by an insulating square of glass 42 inches, made rough and coated with copal varnish, which is placed on the table beneath the lower conductor, its three short feet being let into the table. This square of glass may be used on several occasions as an insulating stand.

The conductor described.

The escape of electricity to a glass plate placed on the table beneath the conductor.

The four rubbers are made of dry walnut wood soaked in amber varnish, and are  $5\frac{3}{8}$  inches long,  $1\frac{1}{2}$  broad, and a little more than  $\frac{1}{4}$  of an inch thick. The metallic plate that communicates with the leather covered with amalgam, is only an  $1\frac{1}{2}$  inch broad, and is fixed externally to the centre of the piece of wood. The rubbers are pressed toward the glass by means of a spring. They are covered with a piece of thick woollen, upon which is a piece of fine neat's leather. After the leather is fastened to the wood, it is wetted, and pressed between

The rubbers described.

\* This varnish is composed of copal, two ounces; gum sandrac, one ounce; white amber, one ounce; olibanum, half an ounce; powdered and dissolved in one pound of alcohol by digestion in a glass matras. When the solution is perfectly cold, it is passed through the filter.

Amber varnish for coating the plate.



two boards, where it is kept till it is again dry. Thus it is rendered very flat, and its edge very sharp, and all its parts will apply to the surface of the glass. This piece of leather is covered with another a little broader, the rough surface of which is towards the glass, and its lower edge on the side towards which the plate moves; and its upper edge on the other side from which the plate moves, being likewise very sharp. The piece of silk is applied with accuracy to this leather. Before it is fastened on, it is heated, and besmeared first with butter of cacao, then with a large quantity of Kienmayer's amalgam\*; and after it is fastened on, it is compressed in conjunction with the wood, or pressed strongly against the machine. The leather is next covered with amber varnish, amalgam is spread over this, and after the varnish is dry, it is smoothed with a burnisher. This is repeated several times. The whole being very dry, and the rubber being pressed so as to touch the glass in all points, the leather coated with amalgam is covered with a piece of fine white paper, as long as the leather, and half an inch broader, so as to cover the seam that fastens the silk to the leather, and the paper is fastened to the wood above or below, accordingly as it is on the ascending or descending side of the plate.

The rubber covered with fine white paper.

What led to this.

Its advantages.

The glass not rendered dull,

does not become streaky,

and neither it nor the silk is soiled. Cleanliness of the whole machine advantageous.

Dry paper is known to be capable of acquiring a high state of electricity, which induced me to try this substance as an immediate rubber. The following are the advantages, that by my experiments, repeated and varied in a great number of ways, I have found paper employed as a rubber to possess over every other known substance.

1. The glass is not rendered dull by the friction, as happens at length, and by frequent using, when it is in immediate contact with the amalgam.

2. By the immediate contact of the amalgam, the glass frequently contracts streaks here and there, that occasion a circulation of the fluid. This cannot take place in the construction I propose.

3. Neither the glass nor the silk can be soiled. It is well known, that the cleanness of the glass, as well as of the rubber and the whole machine in general, is of importance in

\* I add to this amalgam as much silver, as the mercury can dissolve in conjunction with the zinc.

producing an intense degree of electricity. It is true, that it has been proposed to apply the amalgam to the glass instead of the rubbers; but the greater effect, that seems to be produced by this last method, is only apparent, and consists entirely in the circulation of the fluid on the glass, while far from exciting or accumulating more of the fluid, this process and the circulation disperse it.

Rubbing the plate with amalgam instead of the rubbers a bad method.

4. The amalgam on the leather does not require to be frequently renewed. The dust of the amalgam, that is deposited on the edges of the paper, is injurious only when accumulated there in sufficient quantity to be conveyed to the glass, from which however it may easily be removed.

The amalgam does not require frequent renewing.

5. The return and passage of sparks to the rubbers are rendered more difficult, as the paper sufficiently covers the borders of the rubbers, that are turned toward the axis.

The return and passage of sparks to the rubbers more difficult.

6. In my construction the rubbers may be larger than in the usual way, and in reality they are larger in proportion in my machine than in Van-Marum's. No spark passes the axis, unless the air be very damp; for the fluid, in case of a strong accumulation, flies in preference to the opposite rubber over a quarter of the periphery of the glass. I am persuaded, that, by adopting my construction, the rubbers of a plate of 32 inches, such as Van-Marum's is, may be eleven inches instead of nine, in which case there would still be two inches for the diameter of the piece of wood that fastens the plate to the axis, and three inches for the distance from this piece to the rubbers; which I think would be sufficient in these circumstances; and the friction being on a larger surface of the plate, the effect must naturally be much greater. I shall try this alteration of the rubbers on large plates of Bohemian glass, as well as on English cylinders of 18 inches diameter, and 21 inches long. The result I have already obtained with a small cylinder gives me reason to hope much more complete success with a large one.

The rubbers may be longer.

Sparks do not pass to the axis.

Farther trials intended.

7. With my rubbers the friction may be rendered much greater, than with those the amalgam of which is in immediate contact with the glass, and soils it; besides, the plate turns with an uniform friction.

The friction may be made much greater, and is uniform.

8. The activity of the machine is extraordinarily increased by this construction. The greater freedom with which the plate

The activity of the machine extraordinarily increased, and why.



plate moves, even under a greater pressure, and the paper's preventing the glass from being soiled, would be sufficient to produce this effect; even if the greater pressure alone did not occasion a more powerful effect than can be obtained from common machines.

## IX.

*On the Cultivation of the Sunflower, and its Advantages\*.*

The sunflower recommended to cultivators.

THE sunflower, *helianthus annuus* L. is said to have been brought to Europe from Peru. There are two species, one annual, the other perennial: of these the latter is only ornamental, but the annual is of such use in agriculture, that it deserves to be made known to our readers.

Seeds,

have two varieties.

The seeds are white, gray, or blackish; but this difference of colour is entirely accidental, as seeds of either colour produce the others reciprocally. There are two varieties, however, that appear to me permanent, the sunflower with a single stem, and that with a branchy stem, which is less common.

Method of cultivation.

The seed should be sowed in spring, when nothing is to be apprehended from the frost, that would destroy the young plants. The ground should be well broken and manured, if you would have an abundant crop. You may sow broadcast, and afterward thin the plants: but it is better to dibble the seed, placing two in a hole, the holes a foot distant, and the rows two feet asunder. If both seeds germinate, the weaker plant should be pulled up. The plants should be weeded, and the earth dug up between the rows at a proper time. The height of the plant will be from six to nine feet: the stalks are large, some of them being seven or eight inches in circumference near the ground. It flowers in July or August, and the seeds are ripe in Autumn, at the same time with Indian corn. Rainy seasons destroy many of the plants: the foot of the stalk rots in the ground, the leaves suddenly dry up, the stalk breaks off at the root, and the plant dies. A few sunshiny days stop the progress of this disaster.

Rainy seasons bad.

\* *La Decade Philosophique*, No. 26. June, 1803. p. 507.



The leaves of the sunflower furnish abundance of agreeable fodder for cattle: they are gathered in succession without any perceptible injury to the plant; and after this crop of excellent fodder, you may expect another of seeds, that is very abundant. Some stalks afford not less than ten thousand. The best mode of gathering them is to cut the flower stalks, and, as the calyx is very thick, to hang them up in an airy place, that they may dry speedily.

The leaves are excellent fodder.

Mode of gathering the seed.

When they are in flower bees flock to them from all quarters to gather honey.

Bees fond of the flowers.

The seed is rather farinaceous than oily, which they who have attempted to express oil from them did not consider. It is true, that oil may be extracted from them, but in quantities too small to make it worth while to cultivate the plant for this purpose.

The seed affords oil, but in small quantities.

But if the seeds of the substance be incapable of affording oil with advantage, they are valuable for feeding animals commonly kept in the country. They are perfectly well suited to sheep, pigs, and other animals; but they produce a more striking effect on poultry. For these no food is more profitable, or occasions them to lay more eggs.

Good food for sheep, pigs, &c.

particularly for poultry; promoting their laying.

The dry stalks burn well, and afford very good ashes for lye, because they contain a great deal of alkali. In short, from the ease with which it is cultivated, the abundance of its produce, and the change it makes in crops, the sunflower may be considered as a new source of wealth to the farmer.

The stalks burn well, and afford much alkali.

## X.

*Chemical Examination of a new Vegetable Salt, and of a new Acid discovered in it: by KLAPROTH\*.*

THE vegetable production, which forms the subject of the present examination, affords a fresh proof, that hot climates have not only a great influence on the elaboration and particular modification of a great number of vegetable substances, but that they cause productions to be formed, the least vestige of which is not to be found in the same plants in the northern regions.

Hot climates modify the vegetable products of cold, and create new ones.

\* Scherer's *Allgemeines Journal der Chemie*, 1803. No. 55. p. 1.  
Vol. VII.—FEBRUARY, 1804.

- A saline mass exuded from the trunk of a mulberry tree in Sicily. This production consists of a saline mass, excluded from the trunk of the white mulberry, *morus alba* L. which was observed and gathered by Mr. Thompson, in the botanical garden at Palermo.
- External characters. The saline mass in its native salt, as found on the surface of the bark, has the appearance of a coating in little granulous drops of a yellowish and blackish brown. The substance of the bark is equally penetrated with this salt.
- Taste. The first property by which it is distinguished is its taste, which comes nearest to that of the succinic acid.
- Effect of heat. On burning coals this salt swells up slightly, emitting a vapour scarcely perceptible to the eye, but irritating the organ of smell, and leaves a slight earthy residuum.
- Lixiviation affords a light, pale salt, irradiating needles; not deliquescent. To separate the salt, six hundred grains of bark loaded with it were cut off, and lixiviated with a sufficient quantity of water. The lixivium, which was of a brownish red colour was filtered, and evaporated for crystallization; when three hundred and twenty grains of a light salt were obtained, resembling in colour a pale wood, and composed of short needles united in radii, and not attracting the moisture of the air.
- Little soluble in water, though soon crystallizable. Though these crystals do not form till the lixivium is greatly condensed by evaporation, the salt, nevertheless, belongs to the class of those that are little soluble; for 1000 parts of water dissolve only 35 parts with heat, and 15 parts cold.
- Contains no sulphuric acid. The solution of this salt was not rendered turbid by the acetite or the water of barytes, which proves, that it contains no sulphuric acid. The alcaline carbonates precipitate from it, an earth of a brown wood colour, which by a slight heat is calcined to whiteness. This earth dissolves with effervescence in nitric acid, and was precipitated from this solution in the form of sulphate of lime by means of sulphuric acid; and, in that of oxalate of lime by oxalic acid.
- Precipitates acetite of lead, and nitrate of silver and mercury. The solution of the salt readily precipitates acetite of lead, and the precipitate formed is reduced on burning coals. The nitrate of silver was precipitated by it in a pale brown, light, and shining scales, and the nitrate of mercury in whitish flocks.
- Hence it contains lime, with a peculiar vegetable acid, and extractive matter. These experiments indicate, that the salt in question is a neutral salt, composed of lime and a peculiar vegetable acid. This acid, however, was combined with an extractive matter, which rendered the precipitates, that otherwise would have been white, either of a deep or pale wood colour.



Fifty grains of the calcareous salt were heated to incan-By strong heat  
 descence, in a retort communicating with the pneumatio- it gave hidrogen  
 chemical mercurial apparatus, and twelve cubic inches of and carbonic  
 hydrogen gas mingled with carbonic gas were obtained; that  
 burnt with a strong flame. The bulb of the neck of the re-  
 tort contained an acid liquor, on which swam a fluid brown an acid liquor,  
 oil, both together weighing seven or eight grains. The re- and a fluid brown  
 siduum in the retort consisted of a spongy conglobulated mass oil,  
 of a pale brown colour, interspersed with coally particles, having a spongy  
 and dissolved in nitric acid with effervescence. This residuum,  
 solution, containing lime,  
 being filtered, was precipitated with carbonate of ammonia; and a carbona-  
 ceous matter.  
 when 21 grains of carbonate of lime were separated. The  
 coally matter that remained on the filter was very light, weigh-  
 ed four grains and half, and burned on a test, leaving a re-  
 siduum of a quarter of a grain of calcareous earth.

Another portion of the calcareous salt was precipitated by By carbonate of  
 carbonate of ammonia, and the liquid remaining was eva- ammonia, a salt  
 porated by a gentle heat to crystallize it, when it furnished in long slender  
 a salt in long and slender prisms. prisms was ob-  
 tained.

The liquor left, after the crystals were separated, was ex- The remaining  
 amined by several metallic re-agents. The solutions of silver, liquor precipitat-  
 mercury, copper, iron, cobalt, and uranite in nitric acid, ed various me-  
 and those of lead and iron in acetic acid, were powerfully tallic solutions.  
 precipitated by it. The precipitate of copper was of the  
 colour of verdigris; that of cobalt of a pale reddish colour;  
 that of uranite of a yellowish colour; that of iron of a dull  
 brown; and those of silver, mercury, and lead, of a bright  
 brown wood colour.

The same liquor was rendered slightly turbid after a time And the re-  
 by the water and acetite of barytes, the muriate of tin and agents, perhaps  
 gold, and the nitrate of nickel; but these precipitations might owing to the  
 be the effect of the extractive matter, that adhered to the extractive mat-  
 acid, rather than of a combination of this with the metallic ter, rendered it  
 oxides. slightly turbid.

I shall proceed to the experiments I made with a view of Experiments to  
 obtaining the acid of the calcareous salt pure. obtain the acid  
 pure.

Forty-five grains of the precipitate obtained from the de- The precipitate  
 composition of the calcareous salt, by the acetite of lead, with acetite of  
 were mixed with a scruple of sulphuric acid diluted by a drachm lead mixed with  
 of water. The sulphate of lead was separated, and the liquor sulphuric acid.  
 evaporated. It afforded by crystallization thirty-four grains of Decomposed by  
 acid salt in fine needles of the colour of pale wood. sulphuric acid.



Decomposed by  
Sulphuric acid.

In the same manner thirty grains of the dry calcareous salt were decomposed by twelve grains of sulphuric acid, properly diluted. The sulphate of lime being separated, the same acid salt was obtained.

Has the taste of  
succinic acid ;  
is not deliquescent,  
dissolves readily in alcohol  
and does not precipitate metallic  
solutions.

The taste of succinic acid is still more marked in the acid itself. The acid salt remains dry in the air ; it is easily soluble in alcohol as well as in water. It does not precipitate the metallic solutions like its salt.

In the retort an  
acid liquor came  
over ; prismatic,  
colourless, transparent  
crystals sublimed ; and  
a coal remained.

Twenty grains of the acid salt were slightly heated in a small glass retort. First, a couple of drops of an acid liquor came over, the taste of which was perfectly analogous to that of the concrete acid. Next a concrete salt arose, that adhered flat against the top and part of the neck of the retort, in the form of prismatic crystals, colourless and transparent. A coally residuum remained in the retort.

The acid adheres  
strongly to the  
lime.

A similar saline sublimation was not observable with the calcareous salt : we must therefore conclude, that the acid adheres strongly to the lime, and cannot be separated from it by heat, without being decomposed.

The acid washed  
out and crystallized.

To separate the sublimed salt from the coally residuum, the whole contents of the retort were dissolved, and the liquor filtered. The solution was perfectly clear, and by spontaneous evaporation deposited the acid salt in colourless crystals.

Sublimation the  
best mode of obtaining  
it pure.

From this it appears, that a gentle sublimation is the best mode of obtaining the pure acid salt, and freeing it from the extractive matter, to which it adheres too strongly to be separated from it in the moist way.

Perhaps may be  
obtained from  
mulberry trees  
in this climate.

The small quantity of the calcareous salt I had left, did not allow me to carry the examination as far as I could have wished, in order to determine the specific characters of the acid. It is possible, that the mulberry trees in this climate may likewise furnish the same salt, and I shall proceed to inquire into this without delay.

A new acid, the  
*moroxylie* ;  
the salt *moroxylate*  
of lime.

The experiments related, however, seem sufficiently to establish the point of the acid obtained, being a new vegetable acid, which comes nearest to the succinic, both in its taste and other qualities. We may therefore provisionally give it the name of *moroxylie acid*, and the calcareous salt containing it that of *moroxylate of lime*.

## XI.

*On Mr. WOOLF'S Invention for equalizing the Action of a Crank, in contradistinction to the Effect of the Fly, in producing an uniform Effect against a variable Resistance. In a Letter from Mr. T. JONES.*

SIR,  
To Mr. NICHOLSON,

I BEG leave to second Mr. Farcy's request for an explanation of the substitute for the fly and equalization of the action of the steam engine, in producing a rotative motion, given in your valuable work for November last: or, I shall esteem it a favour if you can direct me where I can see it at work \*. It appears to me, that the inventor has lost sight of the value of that organ which he intends to supersede, and that he has not applied it in cases, where a machine is required to produce an uniform effect on a variable resistance, in which case the mere equalisation of the action of the power on a revolving crank would have no value at all.

Inquiry respecting Mr. Woolf's substitute for a fly.

The best attempt that I have seen to unite these two important points, is in a steam engine erected by Mr. Hornblower, at Messrs. Meux and Co's. brewery, where the action of the power on the pin of the crank, may have been in the ratio of the sines of each respective arc in its revolution round the arbor of the fly, I say, "*may have been,*" and I wonder it was not, since that mode of communication would have so easily admitted it,—theoretically at least; then indeed, a very light fly would serve to turn the crank past the upper and nether part or points of the circle; but it could not have answered in regulating the unequal resistance which is found even in grinding malt, or pump works: and in my humble judgment, the fly must for ever keep its place and importance with every judicious mechanic.

Another attempt at the same effect.

I am, in much bodily indisposition,

SIR,

Your humble servant,

T. JONES.

Surry Road, 7th January 1804.

\* If there be one at work near London, I have no doubt but Mr. Woolf the inventor, who is resident engineer at Messrs. Meux's brewery, will readily shew it. N.

Farther



*Farther Remarks on Mr. Woolf's Rotatory Apparatus.*

Farther account  
of Woolf's rota-  
tory apparatus.

IN order to render the effect of Mr. Woolf's contrivance (Fig. 1. Plate XI. Vol. VI. November 1803.) more intelligible to my readers in general, I shall here explain the same in a popular way.

I will suppose, by way of simplifying the subject, that the rod B is so long as to act always nearly in the perpendicular, and that the action upon the small wheel is also in the same direction, namely, constantly that of gravity. The engraver has omitted to continue the rod of the piston supposed to work in the barrel G, and the proof came too late for it to be altered in time. The effect at the pin F in the little wheel, is precisely the same as if a weight was continually hanging there.

Effect of the  
power and  
weight in diffe-  
rent positions.

1. In the position here shewn, the power of the engine does not act at all, and the weight at F has its full operation.
- 2. When C has moved through half the quarter circle to the right hand, F will have arrived at the lowest point; consequently the power of the engine will act by a lever equal to the sine of  $45^{\circ}$ , or seven-tenths of the radius: or seven-tenths of the full power of the engine will then act on the great wheel, and the weight at F will not act at all.
- 3. When C has moved through an entire quadrant, the power of the engine will be the greatest possible on the great wheel, but at the same time the weight will have arrived at the horizontal line beneath E, and will oppose the former action. The motive force will therefore be equal to the excess of the power of the engine beyond the weight.
4. When C has moved through another half quadrant, the action on the great wheel will be equal to seven-tenths, as in No. 2. and the weight, having arrived at the highest point will not act at all.
5. When C has moved through half a circle, and is at the lower station in the great wheel the engine will not act, and the weight having arrived again at F. will have its full operation as in No. 1.

There are four  
positions in  
which the coun-  
ter-weight has  
no effect;

By pursuing this simple method of examination, we see that 1. there are four positions in which the arm C standing at an inclination of  $45^{\circ}$  to the horizon, and the arm F perpendicular to the same, the regulating weight can have no effect,



effect, whatever be its measure; the action will be effectual to seven-tenths of the direct or entire action of the arm B in the tangent to the great circle; 2. that when the arm C is either perpendicularly up or down, the regulating weight alone acts in the same tangent; 3. that when C lies horizontal, the effective force will be the excess of the power of the engine beyond that of the weight. Since therefore we can do nothing with regard to the four first mentioned positions, we have only to consider the four last, and to contrive that the moving force shall be the same at the top and bottom, as at the two sides; or in other words, that the weight alone shall be equal to the excess of the power of the engine beyond that weight; which is the same thing as saying, that the weight must be equal to half the power of the engine.

two in which the weight acts by itself; and two in which the effect is produced by the excess of the power beyond the weight.

The weight must be half the power.

We see then that the action on the wheel C, will vary from seven-tenths to five-tenths, four times in every revolution; the extreme difference being therefore two-tenths each way from the medium, which is about six-tenths. It may not perhaps be of any practical value to discuss the gradation by which the increase and diminution of the opposite actions are governed; and the mathematical reader will easily see that the direct action of each is as the cosine of the angle of the obliquity of the radius it acts upon. Still less does it seem to be of any consequence in the present loose view of the subject, to treat of any assumed obliquity of the rods of communication.

Variation one tenth on each side the medium action.

If we suppose a power to act constantly and uniformly, but with alternations of opposite direction, in equal times and through like spaces; and wish to produce, by that action, a rotatory motion, we might in theory obtain the effect by a double spiral, known by workmen by the name of a snail: but the difficulties from friction, snail, snake, and other impediments, would in many instances render the construction ineligible. The crank is the most cheap, easy, light, and practicable means of producing rotation from alternate right-lined motion and the contrary; and its properties are too well known to require discussion in this place. Its inequalities demand a fly, whether the power or the resistance be uniform or variable; and I do not see any striking difference between Mr. Woolf's contrivance

Concerning the equalization of actions, &c.

Crank.

trivance and the fly, as far as concerns one single revolution, whether they be considered as regulating any one of these. In both, the surplus power is employed in putting a weight into a situation or state by which it is enabled to give back (in theory) the same surplus, when the resistance becomes greater or the power less. But if the whole power, in several successive revolutions, should exceed the resistance, and produce acceleration, the fly will continue to accumulate momentum, and will be ready to give it out, against an augmented resistance in succeeding revolutions; so that its equalizing power extends not only through each single revolution, but through many. I do not perceive this property in the ingenious contrivance before us.

## XII.

*Observations on the extinct Volcanoes in the Environs of Coblentz, by the Chevalier DE SADE. Translated from the original Manuscript, communicated by the Count de BOURNON.*

Coblentz built  
of volcanic stone,

from the neigh-  
bourhood of  
Andernach.

The Roman le-  
gions said to have  
been driven from  
their camp near  
Andernach, by  
a volcano.

This question-  
able.

The extinct vol-  
canoes, are like  
those of Au-  
vergne, of un-  
known date.

Beyond Ander-  
nach, river se-  
diment inter-  
mixed with pu-  
mice stones and  
volcanic sands.

Agemberg, a  
small mountain  
on the west,

WHEN I resided at Coblentz in 1791, the volcanic stones made use of in that city, led me to inquire whence they were procured. I was soon informed, that they came from the neighbourhood of Andernach; and the clergyman of that place acquainted me, that the extinct volcanoes were near Haach, an abbey of Benedictine monks, three leagues distant. This worthy gentleman added, that he had somewhere seen it mentioned, that the Roman legions, encamped near Andernach, were obliged to quit their post, on account of the fire issuing from the volcano. Its great distance from the sea, made me doubt the truth of this; and my own observations have convinced me, that the extinct volcanoes of this country are at length as ancient as those of Auvergne, the date of which is unknown.

Having procured a guide, and directing my steps toward the south, I had scarcely quitted Andernach, before I met with river sediment, intermingled with pumice stones and volcanic sands. Leaving the high road, I proceeded toward a small mountain on the west, called Agemberg.

To



To my great satisfaction, after having traversed pumice stones and volcanic scorix, I arrived at the foot of a quarry of lava, from which hewn stones for building and millstones were taken. This quarry was wrought in the open air, and was covered with a bed of mould about eighteen feet deep. The thickness of this stratum demonstrates the high antiquity of the volcano, while at the same time it prevented me from tracing the course of the lava, particularly as above this lava, nothing is to be found but scattered blocks and fragments of lava, scorix, and pumice stones. I went to the top of the mountain, however, in hopes of discovering where the center might have been; for the mountain of Agemberg, all the rocks of which are torrefied, and the state of scorix, was not a crater. The mountains, by which it is surrounded on the west, appeared to me to be of the same nature, without any indication of the mouth from which the fire issued.

The summit of mount Agemberg is very bare, but on descending a few paces, I found a very pleasant copse. A narrow path, which I took at a venture, led me, after some twenty steps, to a grotto, in which was a tomb, with an inscription from Job, reminding man of his nothingness. By the side of this grotto is another, inhabited by a hermit, with a table of volcanic stone in the centre, which he uses for different purposes. A little higher up, I discovered a third, smaller than either of these, containing a rustic altar; on one side was a pigeon-house, and lower down, two or three little basons of water bordered with shells.

I quitted this mountain with regret, to traverse another, which, though covered with wood, seemed to indicate the existence of a crater; but I could perceive no trace of one, the trees, and the thickness of the mould, letting me see nothing but scattered blocks of lava and scorix.

My examination was soon finished, and I resumed the road to the abbey of Haach. In this I found nothing but pumice stones, lava; in some places of the basaltic kind, scorix, a few fragments of hornstone schistus, and blocks of quartz; all scattered about on a stratum of earth, which itself was composed merely of river-sand and pumice stone.

A quarry of lava, from which hewn stones and millstones were procured.

Wrought in the open air, and covered with eighteen feet of mould.

Above it scattered blocks and fragments of lava, scorix, and pumice stone.

Agemberg not a crater, and the mountains, surrounding it, of the same nature.

The top of Agemberg bare. A little way down a pleasant copse, in which is a grotto, containing a tomb, another inhabited by a hermit, a third in which is an altar, a pigeon-house, and two or three basons of water.

Another mountain covered with trees and deep mould, with scattered blocks of lava and scorix.

In the road to the abbey of Haach, pumice stones, lava, in some places basaltic, scorix, hornstone, schistus, and blocks of quartz, on a stratum of river sand and pumice stone.

The



The crater said to be a lake near. Its situation fine.

On the west side first argil of different colours, then hornstone, next scattered blocks of lava.

The bank a poor meadow.

On the north, blocks of lava, then hornstone cleft perpendicularly, and lastly, a fat, white clay, interspersed with blocks of lava and hornstone.

On the east, compact and porous lava, and the mountain nearly perpendicular.

On the south a level meadow, and a shore covered with shells.

The prevailing winds N. and N. W.

The lake has no apparent influx of water, but a rivulet runs from it on the south.

Varies greatly in depth, more than 400 feet near the middle. Abounds with fish, but the bottom too rocky to use nets.

The abbey built by the count Palatine in 1093, he being alarmed by the nocturnal appearance of fires on the mountains.

The incumbent of the village belonging to the abbey, informed me, that the crater, of which I was in search, was a lake near his parish.

The situation of this lake displays much grandeur. It is surrounded by a chain of mountains covered with trees; its shape is oval, longest from north to south; and it requires near two hours to walk round it.

The west side offers to view at first nothing but a spacious bed of argil, divided into bands of different colours; after this appears a smaller bed of hornstone; and the rest exhibits only some scattered blocks of lava. The bank is an indifferent meadow.

The north side begins with some of the blocks of lava just mentioned: next appears a pretty extensive bed of hornstone, the very numerous fissures of which are uniformly perpendicular to the horizon; and a little farther on, is a considerable bed of fat clay, as white as snow, interspersed with blocks of lava and hornstone.

On approaching the east shore, nothing is to be found but lava, in some places compact, in others porous, and the mountain there is nearly perpendicular.

Lastly, on the south side, the ground becomes level, and is simply a meadow. The strand is covered with shells, partly whole, partly broken; and hence I conclude, that the prevailing winds in this district, are the north and north-west.

This lake does not appear to me to receive water in any part: yet, on the south it has an outlet, that furnishes a small brook, watering the grounds of Medermich. The depth of the lake varies greatly, being upwards of four hundred feet near the middle. It abounds with fish, particularly pike; but the blocks of stone scattered over its bottom, render the use of the net nearly impracticable, so that the line is almost exclusively employed for catching them.

The abbey, to which this lake belonged, was a spacious building, proclaiming the opulence of its founder. It was built in 1093, by Henry count of the Palatinate of the Rhine, and lord of this lake. Broverus, in his annals of Treves, says, that the phantoms and lights, which this prince saw every night on the mountains around him, induced him, to erect this pious foundation.

I was told, that the outlet I have just mentioned was artificial, and made fifty years after the building of the abbey. The foundations of this abbey, having been sunk as deep as the outer walls are high above ground, had obstructed the natural outlet, which occasioned the water to rise so high, as to inundate the convent, and render it necessary to form the present channel, in order that it might run off.

The face of the ground, however, makes me doubt, whether the lake ever flowed over on the side next the abbey; for to a considerable distance it rises there uniformly as you proceed from the shore. It appeared to me, that the natural outlet must have been rather on the side toward Andernach, through fissures on the mountain. Neither can I believe, that this lake, if we may judge from its banks, was the crater of a volcano; but one of the great excavations produced by some earthquakes. In fact, I have already said, that the east and north-east parts of this lake were volcanic: if therefore we suppose, that the summit of the mountain had been a volcano, its base must necessarily have become hollow, and in this state of things, a very slight shock would have sufficed to break the arch of the vault, or the crust that covered its abysses.

We need not wonder, that this lake is always full of water, though it appears to receive none in any part; since it serves as a drain to the surrounding mountains, which are in great measure volcanic. Its water is very limpid, and it does not freeze except in very severe frosts. Marquart, in his origin of the Palatinate, asserts, that it was once larger than it is now; which in fact is perceptible on the south and west, where the ground has the appearance of having been once covered with water. He adds, that precious stones and sapphires were found on its borders. I did not see any; but it is known, that there is a fact of this kind in the little rivulet of Pezzouliou, that runs among lava at Expilly, near Puy, in Velay.

By no means satisfied with my researches, the principal object of which was, to discover the crater of the volcano, that could have produced all the lava in this country, I repaired to Medermenich, half a league to the south-east. There I found myself in quarries of volcanic stone, from which were taken mill-stones and hewn-stones for building,

The present outlet artificial,

the natural one having been obstructed by the foundations of the abbey.

This questionable,

as its natural outlet was more probably toward Andernach, through fissures in the mountain. Neither was it a crater, but probably produced by an earthquake.

Receives water from the surrounding mountains.

Very clear, and does not easily freeze.

Formerly larger than it is now.

Sapphires and other precious stones, once found on its borders. A similar occurrence at Expilly.

At Medermenich, quarries of volcanic stones.

for



for which there is a very great demand. These quarries are underground, and in these are shafts a hundred feet deep, to facilitate the working.

Depth of the strata.

The first stratum was mould, about 15 feet thick.

The second, scorix of lava, about 12

Total 27.

Descent into them.

Beneath was the pure and compact lava. The descent into these quarries is by a stair of 120 steps, and a gentle descent of near 90 paces. The fragments of the stones got out served to make internal walls of support, to which were added stanchions of timber, in some places perpendicular, in others oblique, but always contrived to support the blocks separated by fissures. The stones were broken off by iron wedges, for they were afraid to separate them by blasting.

The roof supported by walls and timber.

The stones separated by iron wedges.

The shafts numerous, and very dangerous from being left open.

These quarries wrought for several centuries.

I counted eighteen or twenty shafts in the space I traversed; and their mouths being perfectly open, woe to the traveller who should lose his way among them by night. I observed several of them covered up, indicating works abandoned; whence we may presume, that these quarries had been wrought for several centuries. Some old oaks likewise growing in the shafts thus covered confirm this opinion.

Its crater probably at mount Blemberg. Beyond Medermenich a bed of the same lava, with a perpendicular face, forming a ravine 25 feet deep, and covered with a thin stratum of mould.

The thickness of the strata covering this lava prevented me from following its course, and tracing it to its source; but I judged, that the mountain of Blemberg, a league to the south-west, might be its crater. Accordingly I proceeded to examine it. A quarter of a league from Medermenich I found a bed of the same lava, the face of which was perpendicular, and forming a gully, that might be twenty-five feet deep. It is covered by a very thin stratum of mould, and is wrought into millstones, and stones for building. Being exposed to the open air, it is harder than that of Medermenich.

This too wrought, and the stone harder from its exposure to the air. Beyond this, and some lava of the piperino species, is a cold acidulous chalybeate spring. More lava in which were formerly quarries,

After having passed this gully, and a little bed of muddy lava, of the nature of piperino, we find in a meadow a very copious mineral spring. It is cold, and contains carbonic acid and iron. At some distance from this we meet with beds of lava, in which there had anciently been several quarries, and at length we arrive at the mountains of Blemberg. The surface of this mountain, though covered with trees on the east, is nevertheless full of torrefied and scorified lava, and gray and red pouzzolona, which left me no doubt, that this mountain had formerly been a volcano. The inspection of its summit



confirmed me in this opinion. Though divided into several parts, which do not perfectly correspond with each other, that on the west perfectly represented a segment of a vast crater in good preservation. Standing on the border of this segment, I observed on the east, at the distance of half a league, quarries of lava, of which this mountain was the source. The gully I have mentioned seemed to militate against the adoption of such an opinion with respect to the quarries of Medermenich, which are to the north-east beyond these: but time, and the various circumstances, that have formed fifteen feet depth of mould, may have occasioned the apparent interruption of this current of lava. I do not doubt however, but several of the neighbouring mountains have been openings of volcanoes.

From the summit of Blemberg I enjoyed a grand and very extensive prospect. On the south I had the little town of Mayerne, on the north Medermenich, and Ettringen on the west.

I quitted this mountain to visit that of Zimmeray, about half a league west of it, and I passed through the village of Ettringen, built of lava and scorice.

The whole of the body of the mountain of Zimmeray is volcanic. The lavas there are in a state of torrefaction, that announces the existence of a crater. In fact, when I reached its summit, I there found all the marks of one. It is of a conical form, and has a hollow opening to the south. The sides of this hollow facing the south are well preserved: those that face the north have been destroyed. I imagine this took place at the period of the last eruption, and that a stream of lava would be found by digging on that side.

I then left this mountain, and proceeded to that of Calberg, which is on the north, and separated from it only by a tolerably broad valley, in which I found trapp, of a blackish gray colour, in mass, and in scattered blocks. At the foot of mount Calberg was a bed of black, coarse grained, volcanic sand, stratiform, and containing a great deal of mica in large leaves. The trees that covered its summit prevented me from ascertaining its nature, but the circumference of its base is entirely volcanic. Around it is muddy lava, in which larger or smaller fragments of compact lava occur. Gray clay predominates there.

Blemberg covered with trees on the east; but abounds with lava and pouzzalana.

Its summit once a vast crater, but partly destroyed.

Quarries of lava that proceeded from it.

As probably did those of Medermenich.

But several of the neighbouring mountains were once volcanoes.

Prospect from Blemberg fine.

Village of Ettringen built of lava and scorice.

The mountain of Zimmeray volcanic.

Its summit a crater.

Mount Calberg contains trapp.

At its foot a bed of volcanic sand,

containing mica in large leaves.

Its base entirely volcanic.

Gray clay predominates.

**Once a volcano.** I had no doubt, that this mountain was once a volcano, its figure alone proves it. I went beyond it to the quarries that were working, and which were to the north, at the foot of a gentle declivity, separated from the Calberg by a valley, through which a small brook flowed to the east. These quarries are parts of vast beds of that kind of lava, which the Italians call piperino. It is in general an earthy lava, containing a pretty considerable quantity of fragments of compact lava. This stone is soft, and easily wrought. It is almost every where in a horizontal stratum, but varies in its texture; for in some places it appears in the form of a volcanic breccia, in others it has the texture of sand-stone.

**Near it quarries of lava.** of the piperino species;

**Soft and easily wrought;** in some places a volcanic breccia; in others of a sandstone texture.

**Road from Medermenich to Coblenz.** Having accomplished my object, which was to discover the volcanic openings, that furnished the lava of this district, I returned to the Medermenich, where I slept; and the next day I reached Coblenz. The plain I crossed was covered with fragments of quartz, trapp, and pumice stone. Near Oetundung, on the high road, we find rocks of slate, which are working. After leaving this village four gently sloping mountains appear on the left, stretching from east to west, and separated only by narrow valleys. Their figure induced me to visit them; and I found them composed of pumice stones and scorice. The first and second are merely segments of two small craters, the northern part of which has been carried away. The other two, which are covered with trees, appear to me to have formed but one crater of vast extent, the southern part of which is destroyed. The lava of these little mountains is encrusted with an immense number of little crystals of piroxen. This reddish lava contains a pretty considerable quantity of reddish mica in large leaves, frequently half a line thick. Almost at the foot of the easternmost mountain this stone is procured, to be used in constructing walls, though it is too porous, and at the same time too hard, to be employed as hewn stone.

**Their lava encrusted with small crystals of piroxen.** It is reddish, and contains reddish mica in large leaves. Used in walls.

**View from these heights.**

From the summit of these heights the prospect is beautiful and grand. From the east to the north appears the magnificent basin watered by the Rhine from the castle of Ehrenbreitstein to Andernach. The seven mountains (Siebenberge) raise their ancient heads on the north. On the west, the view includes from north to south all the mountains just described; and



and on the south it is terminated by mountains covered with wood, the nearest of which appeared to me to be volcanic.

At length I arrived at Basnheim, remarkable for nothing but the English gardens belonging to the lord of the manor; and thence to Coblentz. I found pumice stone on the road, and in strata where pits had been dug in the ground.

English gardens at Basnheim.

Pumice stone on the road and in strata.

### XIII.

*On an Improvement in the Form of Spectacle-Glasses, by WILLIAM HYDE WOLLASTON, M D. F. R. S. Communicated by the Author.*

IT must have been remarked by persons who make use of spectacles, especially those who require glasses of short focal distance, that objects seen through them appear distinct, only when viewed through the central parts of the glasses; that when the direction of the sight  $EO$ , Fig. 1. page 145 is considerably inclined to the surfaces, objects appear distorted, and that this defect is greater in proportion to the greater obliquity of that line.

Imperfection of spectacles, that objects out of center are distorted.

It is on this account that opticians have lately made, and recommended spectacle-glasses of less diameter than those formerly in use, thinking that the extreme parts of the field of vision, which from indistinctness were of little use, might be spared without much inconvenience. But this alteration in the size of the glasses could hardly claim the merit of an improvement; since for one defect, it only substituted another scarcely less objectionable.

Remedy formerly proposed; to make the glasses smaller;—

It seems indeed rather extraordinary, that, during five centuries which have elapsed since the invention of spectacles, neither theory nor accident should have produced any considerable variation from the original construction.

It was indeed conceived by Huygens, that the glasses, instead of being equally curved on both sides, as is customary, should have the curvatures of their opposite surfaces in the proportion of six to one, because he had demonstrated that such a form was best suited to the object glasses of telescopes.

but to little effect.

Huygens proposed that the radii of curvature in spectacle glasses should be unequal.

Dr. Smith also in his treatise on optics, (p. 258.) repeats this opinion of Huygens in the following cursory manner;

Repeated by Smith.

and



“ and consequently this figure of a glass is the best for spectacles, as the double concave of like figure is the best to help short-sighted persons.”

This is good for simple object lenses, but not for spectacles.

But although it may be very true that such a form of glass was best calculated for the object glass of a telescope, previous to the celebrated discovery of the achromatic object-glass by the late Mr. Dollond, yet whatever advantages might at any time be expected from the telescopic object-glass so shaped, these were not to be obtained by a similar construction in spectacles, as may easily be seen by considering the different uses of the respective instruments.

Telescopic vision is nearly in the axis:—

In a telescope in the first place, our view is necessarily confined to a very small distance on each side of the axis; and secondly, every part of the object glass contributes to the distinctness of any object viewed.

and the whole lens acts for every radiant point.

It is under these circumstances alone that the proportion of the curvatures above-mentioned, might be proper for a single object glass, as being capable of collecting into the focus the rays that fall on every part of it parallel to the axis.

In spectacles it is very different.

By spectacles on the contrary, objects are to be viewed if possible in every direction in which they might be seen by the naked eye, which is often far removed from the centers of the glasses; consequently a constitution that is calculated to represent correctly central objects alone, cannot be the most advantageous.

A small part of the lens only is employed at once.

In these also, the portion of the glass employed at once is scarcely larger than the pupil of the eye, so that any endeavour to procure the concurrence of all parts of a glass in any one effect is evidently superfluous, and may also be shewn to be prejudicial.

Whence improvement must be sought from different considerations.

It is therefore proposed to remedy the imperfections observable in the spectacle glasses hitherto generally used, upon a principle suggested by this latter consideration, which presents an opportunity by a different construction of rendering objects in all directions distinct.

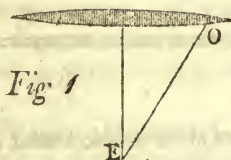
If the lens be made concave next the eye all the rays may be had to pass, through nearly perpendicular.

The alteration requisite for this purpose, is extremely simple, and easily intelligible. Supposing an eye to be placed in the center of any hollow globe of glass, it is plain that objects would then be seen *perpendicularly* through its surface in every direction. Consequently the more nearly any spectacle-

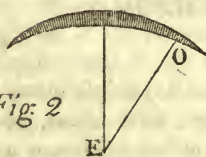
spectacle-glass can be made to surround the eye in the manner of a globular surface, the more nearly will every part of it be at right angles to the line of sight; the more uniform will be the power of its different parts, and the more completely will the indistinctness of lateral objects be avoided\*.

According to this principle, all spectacle-glasses should be convex on their exterior surface, and concave within. The section of those for long sighted persons, will assume the form of a meniscus or crescent, *Fig. 2.* and those adapted for short sight will have their principal curvature on the concave side, *Fig. 3.*

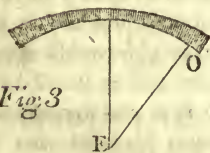
Convex outside  
and concave  
within.



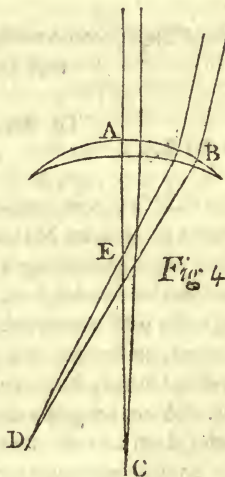
*Fig. 1*



*Fig. 2*



*Fig. 3*



*Fig. 4*

It is only necessary to add, that the advantage of this improvement in the form of spectacle glasses has been confirmed by a sufficient number of experiments on different persons, and that those in particular who are very long or very short sighted, are much benefited by them.

Experience confirms this doctrine.

\* To mathematicians it will be evident that any ray which does not pass through the center of a lens, cannot be at right angles to both surfaces; but they will also perceive that when any small oblique pencil makes equal angles with the two surfaces of a thin lens, the inclination of it to each is so small, that its focal length *BD*, *Fig. 4.* will not sensibly differ from *AC* that of a central pencil.

and a manufactory is established.

The most advantageous proportions of curvature for obtaining the different focal lengths now generally distinguished by certain numbers, have also been duly considered; and the manufacture of spectacles, on this construction, has been undertaken by Messrs. P. and J. Dollond, to whom the exclusive sale of them is secured by patent, and whose well known skill in the construction of optical instruments, ensures to this improvement every advantage of correct execution.

The spectacles are called periscopic.

From the opportunity afforded by these glasses of *looking round* at various objects in one position of the head, it is thought they may not improperly be distinguished by the name of *Periscopic Spectacles*.

#### XIV.

*Letter of Enquiry concerning a Gun to throw double-headed Shot; with the Editor's Reply.*

To Mr. NICHOLSON.

SIR,

Drawing and description of a gun on a paper at the Royal Institution.

HAVING this instant seen, at the Royal Institution, a sheet of paper printed by Nichols, printer, Earls Court, Newport Street, Soho, containing a drawing and description of a gun to throw double-headed shot, I take the liberty of writing this on a page of a little note-book there, which must be my apology for the paper, to entreat that you will observe upon it in your invaluable Journal, because I fear that it is not a safe instrument. A considerable distance is to be observed between the powder fired and the shot to be discharged by it. I have always considered that an interval so kept was the occasion of the bursting of so many guns. Pray is this safe? And should not a line be cut on the arms to which the bullets are attached, to mark exactly the distance of the attaching bar at each extremity from the muzzle, when the balls are equally rammed down. As the knowledge of its safety may contribute to its service if really useful, I hope you will think this application not idle.

I am, Sir,

Your constant READER.

ANSWER



## ANSWER. W. N.

THE paper alluded to in the letter of my correspondent, contains an engraving of the shaded section of a gun having a double bore, but only one chamber; from which each bore directly leads: but they diverge from each other in an angle of about eighteen degrees. The charge of powder is to be placed in the chamber, and the two shot, one for each bore, are connected by a bar having two joints or hinges at an equal distance from its middle. By this contrivance, by bending the compound piece at the hinges, the shot can be put into the bores of the gun; and when the explosion blows them out, their divergence causes the bar to become strait, and consequently is expected to do mischief proportioned to the extent of its length. More particular account.

The title of the paper is, "Horizontal Section of a Gun for projecting double-headed Shot, invented by Lieut. Colonel Blaquiere, 22 Dragoons, July, 1803. Recommended to be used against an invading Army on board his Majesty's Gun-Boats;" and in the directions care is prescribed, that the same length of bar shall be left out of each muzzle to prevent accident; and it is stated to be a great advantage, that a considerable distance should be left between the powder and shot, as by this means the powder acquires its greatest possible force. Title of the paper and instructions.

My opinion was asked concerning a gun of this description which another inventor had proposed, some time previous to Midsummer last, to throw chain-shot; and the observations that then occurred were, 1. If there be the least difference between the windages or the blast in each bore, or in the casual impediments, one shot will precede the other. 2. If they go out fairly together, their velocity of separation will be to the direct velocity as the chord of the angle of divergence is to the radius. In the present gun the balls will separate with one-third of the velocity with which they advance. 3. That the violent separation of the shot from each other, will most probably, in either of these cases, break the chain, or the bar, or the joints. 4. Or if no fracture takes place, the sudden check will, by the elasticity of the metal, cause the shot to approach again to each other. 5. To which I may add, that it may be doubted whether the embarrassment of one of these bar-shots preceding the other might not occasion a dangerous obstruction. This gun invented also by another person.  
Its probable effects.

tion, and that there are many facts which shew, that it is unsafe to permit any considerable expansion of, or space for, the blast of powder, before it shall act on the projectile.

## XV.

*Prognostics of the Weather, established by long-continued Observation upon the Conduct and Appearances of Birds, Beasts, Insects, Plants, Meteors, the Heavenly Bodies, Minerals, &c. Communicated by a Correspondent.*

To Mr. NICHOLSON,

SIR,

Introduction.  
Prognostics of  
the weather.

IT is a well known general fact, that shepherds and others, whose occupations lie in the open air, do not unfrequently possess the skill of foretelling the weather for considerable periods of time in advance, and that they ground their observations upon the phenomena exhibited by animals, and other bodies exposed to the action of the elements. Some of the maxims on this subject, which possess the sanction of ancient acquiescence, are such as probably would not stand the test of modern scientific examination; but on the other hand, there are many of which the rationale appears not difficult to explain, and others which, from their empyrical value, are highly deserving to be studied and made out. I have for these reasons, as well as from a sense of the immediate utility of this knowledge, thought it by no means impertinent to beg that you would oblige the world with the enclosed, which is the best collection of facts I have met with. It is taken from a small pamphlet printed at Edinburgh, without date or the name of bookseller or author, but apparently many years ago, intitled, *A Succinct Treatise of Popular Astronomy*. If this communication should meet the honour of insertion in your excellent Miscellany, I hope it will be followed by some explanations from your scientific correspondents.

I am, Sir,

Your obliged reader,

R. B.

*Signs of Rain from Birds.*

SEA and fresh water-fowls, such as cormorants, sea-gulls, muir-hens, &c. flying from sea, or the fresh waters, to land, shew bad weather at hand: land fowls flying to waters, and those shaking, washing, and noisy, especially in the evening, denote the same: geese, ducks, coots, &c. picking, shaking, washing, and noisy; rooks and crows in flocks, and suddenly disappearing; pyes and jays in flocks, and very noisy; the raven or hooded-crow crying in the morning, with an interruption in their notes, or crows being very clamorous at even; the heron, bittern and swallow flying low; birds forsaking their meat and flying to their nests; poultry going to roost, or pigeons to their dove-house; tame fowls grubbing in the dust, and clapping their wings; small birds seeming to duck, and wash in the sand; the late and early crowing of the cock, and clapping his wings; the early singing of wood-larks; the early chirping of sparrows; the early note of the chaffinch near houses; the dull appearance of robin red-breast near houses; peacocks and owls unusually clamorous.

Prognostics of  
the weather.  
Rain from birds.

*Signs of Wind from Birds.*

Sea and fresh water-fowls gathering in flocks to the banks, and there sporting, especially in the morning; wild geese flying high, and in flocks, and directing their course eastward; coots restless and clamorous; the hoopoe loud in his note; the king's-fisher taking to land; rooks darting or shooting in the air, or sporting on the banks of fresh waters; and lastly, the appearance of the malefigie at sea is a certain forerunner of violent winds, and (early in the morning) denotes horrible tempests at hand.

Wind from  
birds.

*Signs of Fair Weather from Birds.*

Halcyons, sea-ducks, &c. leaving the land and flocking to the sea; kites, herons, bitterns and swallows flying high and loud in their notes; lapwings restless and clamorous; sparrows after sun-rise restless and noisy; ravens, hawks and kestrels (in the morning) loud in their notes; robin red-breast mounted high, and loud in his song; larks soaring high, and loud in their songs; owls hooting with an easy and clear note; bats appearing early in the evening.

Fair weather  
from birds.

*Signs*



*Signs of Rain from Beasts.*Rain from  
beasts.

Asses braying more frequently than usual; hogs playing, scattering their food, or carrying straw in their mouths; oxen snuffing the air, looking to the south, while lying on their right sides, or licking their hooves; cattle grasping for air at noon; calves running violently and gamboling; deer, sheep, or goats, leaping, fighting or pushing; cats washing their face and ears; dogs eagerly scraping up earth; foxes barking, or wolves howling; moles throwing up earth more than usual; rats and mice more restless than usual; a grumbling noise in the belly of hounds.

*Signs of Rain from Insects.*Rain from in-  
sects.

Worms crawling out of the earth in great abundance; spiders falling from their webs; flies dull and restless; ants hastening to their nests; bees hastening home, and keeping close in their hives; frogs and toads drawing nigh to houses; frogs croaking from ditches; toads crying on eminences; gnats singing more than usual; but, if gnats play in the open air, or if hornets, wasps, and glow-worms appear plentifully in the evening, or if spiders webs are seen in the air, or on the grass, or trees, these do all denote fair and warm weather at hand.

*Signs of Rain from the Sun.*

Rain, from sun.

Sun rising dim or waterish; rising red with blackish beams mixed along with his rays; rising in a musty or muddy colour; rising red and turning blackish; setting under a thick cloud; setting with a red sky in the east.

N. B. Sudden rains never last long; but when the air grows thick by degrees, and the sun, moon, and stars shine dimmer and dimmer, then it is like to rain six hours usually.

*Signs of Wind from the Sun.*Wind from the  
sun.

Sun rising pale and setting red, with an iris; rising large in surface; rising with a red sky in the north; setting of a bloody colour; setting pale, with one or more dark circles, or accompanied with red streaks; seeming concave or hollow; seeming divided, great storms; parhelia, or mock suns, never appear, but are followed by tempests.

*Signs of Fair Weather from the Sun.*

Sun rising clear, having set clear the night before; rising while the clouds about him are driving to the west; rising with an iris around him, and that iris wearing away equally on all sides, then expect fair and settled weather; rising clear and not hot; setting in red clouds, according to the old observation: Fair weather from the sun,

*The evening red and morning grey,  
Is the sure sign of a fair day.*

*Signs of Rain from the Moon.*

Moon pale in colour, rain; horns blunt at first rising, rain; horns blunt, at or within two or three days after change, denotes rain for that quarter; an iris with a south wind, rain next day; wind south third night after change, rain next day; the wind south, and the moon not seen before the fourth night, rain most of that month; full moon in April, new and full moon in August, for most part, bring rain; mock-moons are the forerunners of great rains, land-floods, and inundation. Rain, from the moon.

*Signs of Wind from the Moon.*

Moon seeming greatly enlarged; appearing of a red colour; horns sharp and blackish; if included with a clear and ruddy iris; if the iris be double, or seem to be broken in parts, tempests. Wind from the moon.

N.B. On the new moon, the wind for the most part changes.

When the moon, at four days old, has her horns sharp, she foretels a tempest at sea, unless she has a circle about her, and that too entire, because, by that she shews that it is not like to be bad weather, till it is full moon.

*Signs of Fair Weather from the Moon.*

Moon seeming to exhibit bright spots; a clear iris with full moon; horns sharp fourth day, fair till full; horns blunt at first rising, or within two or three days after change, denotes rain for that quarter; but fair weather the other three quarters; moon clear three days after change or before full, always denotes fair weather. After every change and full, rains for the most part, succeeded by fair settled weather; moon clear and bright, always fair weather. Fair weather from the moon.

*Signs*

*Signs of Weather from the Stars.*

Weather from  
stars.

Stars seeming large, dull, and pale of colour, rain; or when their twinkling is not perceptible, or if encompassed with an iris. In summer, when wind is at east, and stars seem greater than usual, then expect sudden rain; stars appearing great in number, yet clear and bright, seeming to shoot or dart, denote fair weather in summer, and in winter frost.

*Signs of Rain from the Clouds.*

Rain from  
clouds.

In cloudy weather, when the wind falls, rain follows; clouds growing bigger, or seeming like rocks or towers settling on mountains tops; coming from the south, or often changing their course; many in number at north west in the even; being black in colour from the east, rain at night; but out of the west, rain next day; being many like fleeces of wool, from the east; rain for two or three days; lying like ridges about mid-day in the south-west, shews great storms both of wind and rain to be nigh.

*Signs of Wind from the Clouds.*

Wind from  
clouds.

Clouds flying to and fro; appearing suddenly from the south or west; appearing red, or accompanied with redness in the air, especially in the morning; being of a leadish colour in the north-west; single clouds denote wind from whence they come; but if at sun-set, clouds appear with golden edges, or diminish in bulk, or small clouds sink low, or draw against the wind, or appear small, white, and scattered in the north-west (such as are vulgarly called mackerel) when the sun is high, these are signs of fair weather.

*N. B.* It is often observed, that though the mackerel sky denotes fair weather for that day, yet for the most part, rain follows in a day or two after.

*Signs of Rain from the Rainbow.*

Rain from  
rainbow.

After a long drought, the rainbow denotes sudden and heavy rains; if green be the predominant colour, it denotes rain, but if red, wind with rain; if the clouds grow darker, rain; if the bow seems broken, violent storms; if appearing at noon, much rain; if in the west, great rain, with thunder.

*N. B.*



*N. B.* It is observed, that if the last week in February, and the first fortnight of March, be mostly rainy, and attended with frequent appearances of the bow, a wet spring and summer may be expected.

*Signs of Fair Weather from the Rainbow.*

The rainbow appearing after long rains, denotes fair <sup>Fair from rain-</sup> weather at hand; if the colours grow lighter, fair; if the bow <sup>bow.</sup> suddenly disappears, fair; if the bow appear in the morning, it is the sign of small rains, followed by fair weather; if appearing at night, fair weather; if appearing in the east, in the evening, fair; if the bow appear double, it denotes fair weather at present, but rain in a few days; if in autumn, it continues fair for two days after the appearance of the aurora borealis, expect fair weather for at least eight days more.

*Signs of Rain from Mists.*

If mists be attracted to the tops of hills then expect rain in <sup>Rain from</sup> a day or two; if in dry weather, they be observed to ascend <sup>mists.</sup> more than usual then expect sudden rain; mists in the new moon always foreshew rain in the old; mists also in the old moon denote rains to happen in the new; a misty white scare, in a clear sky, in the south-east is always a forerunner of rain.

*Signs of Fair Weather from Mists.*

If mists dissipate quickly, or descend after rain, it is a sure <sup>Fair from mists.</sup> sign of fair weather; a general mist before sun rising near the full moon, denotes fair weather for about a fortnight running. If after sun set or before sun rise, a white mist arise from the waters and meads, it denotes warm and fair weather next day. A misty dew on the inside of glass windows shews fair weather for that day.

*Signs of Rain from inanimate Bodies.*

Wood swelling, or stones seeming to sweat; lute or viol <sup>Rain from in-</sup> strings breaking; printed canvas or pasted maps relaxing; salt <sup>animate bodies.</sup> becoming moist; rivers sinking, or floods suddenly abating; remarkable sparkling of lamps or candles; remarkable halo about the candle; great dryness of the earth; pools seeming troubled or muddy; yellow scum on the surface of stagnant waters; dandelion or pimpernel shutting up; trefoil swelling in stalk, while the leaves bow down.

*N. B.* A dry spring is always attended with a rainy winter.

*Signs*

*Signs of Wind from inanimate Bodies.*

Wind from in-  
animate bodies.

Winds shifting to the opposite point; sea calm, with a murmuring noise; a murmuring noise from the woods and rocks; when the air is calm; leaves and feathers seeming much agitated; tides high when the thermometer is high; trembling or flexuous burning of flames; coal burning white with a murmuring noise; thunder in the morning with a clear sky; thunder from the north.

*N. B.* Whensoever the wind begins to shift, it will not rest till it comes to the opposite point; and, if the wind be in the north, it will be cold; if in the north-east colder; if in the south; it brings rain; but if in the south-west more rain.

*Signs of Rain ceasing.*

Rain ceasing.

The sudden closing of gaps in the earth; the remarkable rising of springs or rivers; if the rain begins an hour or two before sun rise it is like to be fair ere noon; but if an hour, or two after sun-rise, it for the most part, happens to continue all day and then to cease; when it begins to rain from the south with a high wind for two or three hours, and that the wind falls, and it still continues raining, it is then like to continue for twelve hours or more, and then to cease.

*N. B.* These long rains seldom happen to hold above twenty-four hours, or happen above once a year.

*Signs of Wind ceasing.*

Wind ceasing.

A hasty shower after raging winds is a sure sign of the storm being near an end. If the water ruckles and frequent bubbles arise, or if the halcyon or kings-fisher attempts the sea while the storm lasts, or moles come out of their holes, or sparrows chirp merrily, these are all certain signs of the storm ceasing. Both sea and fresh water fishes by their frequent rising and fluttering on the surface of the water, foretell the storm nigh over, but especially dolphins spouting up water in a storm foretell a calm.

*N. B.* Let the wind be in what quarter it will upon the new moon, it presently changes.

*Signs of Hail.*

Hail.

Clouds white, inclining to yellow, and moving heavily though the wind be high is a sure sign of hail; if the eastern sky before sun-rise be pale, and refracted rays appear in thick clouds,

clouds, then expect great storms of hail; white clouds in summer are a sign of hail, but in winter they denote snow, especially when we perceive the air to be a little warm; in spring or winter when clouds appear of a blueish white, and expand much, expect small hail or drizzling, which properly is no other than frozen mists.

*Signs of Thunder.*

Meteors shooting in the summer's evening, or chops and clefts Thunder, in the earth, when the weather is sultry, always foretell thunder is nigh; in summer or harvest, when the wind has been south two or three days, and the thermometer high, and clouds rise with great white tops like towers, as if one were upon the top of another and joined with black on the nether side, expect rain and thunder suddenly; if two such clouds arise, one on either hand, it is then time to look for shelter, as the thunder is very nigh.

*N. B.* It is observed that it thunders most with a south wind and least with an east.

*Signs of Cold and Frosty Weather.*

Sea-pyes, starlings, fieldfares, with other migratory birds Cold and frosty, appearing early, denote a cold season to ensue; the early appearance of small birds in flocks and of robin red-breasts near houses; sun in harvest after setting in a mist or broader than usual; moon bright, with sharp horns, after change; wind shifting to the east or north after change; sky full of twinkling stars; small clouds hovering low in the north; snow falling small, while clouds appear on heaps like rocks.

*N. B.* Frosts in autumn are always succeeded with rain.

*Signs of Thaw.*

Snow falling in large flakes while the wind is at south; Thaw. cracks appearing in the ice; sun looking waterish; the moon's horns blunted; stars looking dull; wind turning to the south; wind extremely shifting; it is also observed, that, if October and November be frost and snow, January and February are like to be open and mild.

*Signs of Drought.*

Fair weather for a week together, while the wind is all that Drought. time in the south, is, for the most part, followed by a great drought;



drought; if February be for most rainy, spring and summer quarters are like to be so too; but if it happen to be altogether fair, then expect a drought to follow; if lightning follow after twenty-four hours of dry and fair weather, drought will follow, but if within the twenty-four hours, expect great rains.

*Signs of Hard Winters.*

**Hard winters.**

A moist and cold summer, and mild autumn, are sure signs of a hard and severe winter; store of hips and haws denote the same; the hazel-tree flowering is ever observed to foretell the same; acorns found without any insect is a sure prognostic of a hard winter.

*Signs of Pestilential Seasons.*

**Pestilential seasons.**

A dry and cold winter with a southerly wind; a very rainy spring, sickness in summer; if summer be dry with the wind northerly but the autumn rainy and the wind southerly, great sickness is likely to follow; great heats in spring time without winds; roots having a luscious taste, while the wind has been long southerly without rain; and lastly, great quantities of stinking atoms, insects or animals, as flies, frogs, snakes, locusts, &c.

*Experiments of the Leech Worm.*

**Leech worm.**

Inclose the leech worm in an eight ounce vial glass, three fourths filled with water covered with a bit of linen, let the water be changed once a week in summer, and once a fortnight in winter.

If the leech lies motionless at the bottom in a spiral form, fair weather; if crept to the top, rain; if restless, wind; if very restless, and without the water, thunder; if in winter at bottom, frost; but, if in the winter it pitches its dwelling on the mouth of the vial, snow.

*Signs of Weather from the Barometer.*

**Barometer.**

In calm weather, when the air is inclined to rain, the mercury is low; but when tending to fair, it will rise; in very hot weather when falling, it foretells thunder; if rising in winter, frost; but, if falling in frost, thaw; if rising in a continued frost, snow; if foul weather quickly on its falling, soon over; if fair weather quickly on its rising, soon over; also if rising high in foul weather, and so continuing for two or three days, before

before the foul weather is over, then expect a continuance of fair weather; but, if in fair weather the mercury fall low, and so continue for two or three days, then expect much rain, and probably high winds.

*N. B.* In an east wind, the mercury always rises and falls lowest before great winds.

## XVI.

*A New, Cheap and Simple Apparatus for repeating the Hours, and Quarters in Clocks and Watches. By Mr. J. M. ELLIOT. Communicated by the Inventor.*

THE great expence of repeating watches in the first purchase, as well as in the subsequent repairs, have induced Mr. Elliot to turn his thoughts towards reducing their construction to a greater degree of simplicity, and consequently to diminish the charges in both these respects.

New and simple repeating motion.

In the mechanism represented in plates VII. and VIII. the parts and their arrangement are such that without the usual apparatus of pinions, pullies, chains, or racks, the watches repeat both the hour and the quarters, viz. the hour first and the quarter afterwards, or the hour or the quarters separately; or either of these first as may be wished, without interruption, and by one hammer. As several of these pieces have been made and disposed of in this country as well as exported, the practicability as well as the cost are both ascertained, which last is about one third of the former charges.

Plates VII. and VIII. represent the parts of this new machinery, *Fig. 1.* shews the principal pieces as attached to the upper pillar plate by means of a pottance A B C D, called the repeating pottance, kept to its place by the screw *x*.—*a b* shews the pendant attached to a steel axis upon which the repeating apparatus is fixed and carried. In the actual time piece these parts are as close to each other as convenience will allow; but they are here represented for the sake of distinctness, as if at a distance from each other. Thus they are made to act and perform the striking by turning the axis of the pendant to the right or left.

*c* is a part of the bell seen edgewise, *d* and *l* are two arms or pallets, which being fitted upon squares on the axis are

New and simple  
repeating mo-  
tion.

moved round whenever the axis is moved by means of the pendant. The first *d* serves to drive the work for striking the hour, and the latter *l* the work for the quarters. The first of these apparatuses consists of *c* the hour-locking snail (see also *Fig. 2.*) which is fitted on the axis by a round socket or pipe, upon which socket also is fixed (steadily) the hour repeating wheel *g* (*Fig. 1* and *2.*) and (on a round part) the ratchet wheel *h*, which last is drawn by a spiral spring *k k* in the direction opposite to that which is produced by the pallet *d* (*Fig. 1* and *2.*) *s* is the hammer urged by its spring *v* outwards, and the designer has, I am sorry to perceive, omitted to make a small tail from the center pin near *k* towards *g* the hour repeating wheel. Now the action of striking is simply this; when by turning the pendant *d* acts upon the small pin in *c*, the wheel *g* with its ratchet wheel *h* are carried round, and the teeth of *g* act on the tail of the hammer, and give more or fewer strokes, according to the number that pass.

The number is regulated thus, *a* (*Fig. 4.*) is the star-wheel as usual, and *b* the hour snail screwed on the same; *c* is the click with its back spring, and *n z* is the hour-locking lever, which terminates in an hook at *z* that may be seen in *Fig. 2.* bearing against the hour-locking snail *e*. This hook will evidently be nearer to or farther from the axis of the locking-snail *e*, according to the portion of the other end of the lever which bears against the hour snail *b*, and will consequently stop the pendant in its motion sooner or later by its opposition to one of the stops of the snail *e*. As the star wheel *a* is carried regularly round by the canon pinion as usual, its situation will determine the number of strokes.

The instant the pendant is let go, the spring *k* acts upon the ratchet wheel, and restores the original situation ready for striking again.

After this account of the hours, the striking of the quarters will require little explanation, *l* (*Fig. 1* and *5.*) is the pallet or arm acting (by a reverse motion of the pendant,) on the locking snail *m*, which carries the contrate wheel *p*, for striking the quarters by its action, upon another tail *r* of the hammer; and the original situation is restored by a ratchet wheel *O* and its spring, (see *Fig. 6.*)

In *Fig. 4.* is seen the quarter snail *g* with its lever *h k*, whose hook for regulating the number, may be seen at *D* in *Fig. 5.* and the snail is prevented from going back by a fixed hook.



## SCIENTIFIC NEWS.

*Prize for the Artificial Composition of Palladium.*

IN consequence of the intimation received by the anonymous paper of which a copy is given in the last number of this Journal, at page 75, I waited upon Mrs. Forster, who personally assured me that she holds the sum of twenty pounds, (which was paid to her by the same unknown person who placed the palladium in her hands,) under the trust or engagement to pay the same to the bearer of the certificate mentioned on the said paper, at any time before Midsummer next; and that, if not claimed during that time, she shall afterwards consider herself at liberty to repay the money when demanded by the same unknown person.

I have therefore requested Charles Hatchett, Esq. F. R. S. and Edward Howard, Esq. F. R. S. to join myself as judges of the product which may be made in our presence, agreeably to the before-mentioned paper; to which proposal they have consented. And I have no doubt but that the commission will enable me to present to my readers an account of whatever may be the result of this public invitation.

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*Prize adjudged at the last Public Meeting of the National Institute\*.*

THE decree of the government, May the 2nd. 1803, that authorized the Institute to accept the capital of 10000 franks, [417 £.] offered by Lalande, expresses, "that, agreeably to the intention of the donor, the interest should be laid out annually in a gold medal, which, or its value in money, should be given to the person, in France or elsewhere, the members of the Institute only excepted, who should have made the observation, or written the memoir, that might appear most conducive to the progress of astronomy."

Annual prize for the most useful astronomical paper or observation.

Accordingly, on the report of the committee nominated for the purpose, the physical and mathematical class decreed the prize to Dr. W. Olbers, for having discovered, in the course of the year 10, the planet to which astronomers have given the name of *pallas olberfiana*.

Decreed to Dr. Olbers for the discovery of a planet.

\* *La Decade Philosophique*. No. 29. July 1803. p. 142.

The Dr's. merit  
as an astronomer.

Before this discovery Dr. Olbers enjoyed considerable reputation among astronomers, for a dissertation on the most easy and commodious method of calculating the orbit of a comet; and for the laborious calculations and observations he had made, to re-discover Ceres, or Piazzi's planet, which in fact he did perceive, and was the first to announce to us.

### *Teylerian Society at Haarlem.\**

Teylerian so-  
ciety's prize  
question.

THIS society has proposed the following question:

"Has the application of pretended supernatural principles to physics contributed to the progress of the science? or does not the history of this science prove on the contrary, that all the progress made in natural philosophy has been owing to observations, experiments, the conclusions drawn from them, and mathematical calculations and demonstrations?"

The prize is a gold medal of the value of 400 florins (£40), and the answers to the question, written in Dutch, Latin, French, English, or German, must be sent to the society before the 1st of April, 1804.

\* \* The explanation of the subjects of the queries of R. B. in our last, (page 71.) are deferred for want of room.

\* *La Decade Philosophique*, No. 33. August, 1802. p. 382.

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A  
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AND  
THE ARTS.

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MARCH, 1804.

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ARTICLE I.

*Description of a new Steam Digester for Philosophical Researches.*

*By Sir A. N. EDELCRANTZ, Counsellor of the Chancery, and private Secretary to the King of Sweden; Member of the Swedish Academy, &c. &c. Communicated by the Author.*

THE interesting and curious phenomena which this machine presented, from its first discovery by Papin, have always rendered it a desirable part of the chemical apparatus; but the difficulties and dangers attending its use, have, at the same time, prevented its applications from being so general, as the importance of the object seemed to promise. Many, and some of them successful, attempts have been made to adapt this instrument to economical and culinary uses; but these being commonly limited within very few degrees of heat above the boiling point, such digesters, though they can be used with safety, are at least inaccurate in the permanence, and uncertain in the determination of the elasticity and heat of the vapours acting in each experiment, which consequently cannot be either precise or comparable with others: and though several ingenious philosophers, such as Watt, Betancourt, Smith of Geissen, Van Marum, &c. have in later times very much improved their scientific application, enough of imperfection still seems to remain to prevent their general use, and to afford sufficient scope for further improvements.

Excellence and utility of Papin's digester,

Improvements.



As long ago as the year 1793 I applied my thoughts to render this instrument less imperfect, and though I cannot flatter myself with having succeeded in any considerable degree, a short account of my attempts may not perhaps be thought quite useless.

The usual method of closing the digester is faulty in many respects.

New cover which fits by a conical interior surface, without packing.

Inconvenience of this contrivance.

My first object was to obtain a perfect closure for the digester, which is necessary not only for increasing the heat, but also for preserving the contents. In former contrivances this has generally been effected by a metallic cover pressed on the top of the vessel by means of screws, wedges or other mechanical contrivances, with a circular piece of leather, paper, or other soft material between both. I also tried these methods in a great variety of ways, but always found such covers either leaky, on applying moderate pressures, or when the pressure was stronger, too cumbersome for a course of varied and repeated experiments. Besides which, at the degree of 260 or 270° Fahr. the leather, as observed before by Mr. *Betancourt*, is commonly decomposed or burnt, and linen or paper cease to close at a still lower temperature; as soon as the moisture, they are imbibed with becomes converted into vapour. I therefore used another cover of a particular kind, consisting of a thick circular plate, with its edge turned conical, and ground to fit exactly in the under side of a metallic ring of the same conical form, folded to the mouth of the digester, in which the plate was previously included. When the vessel was ready for experiment and the plate lifted up so as to apply to the ring, it closed as exactly as could be desired, and the increased force of the steam, which tends to render other covers defective, had in this a contrary effect. Two difficulties were thus overcome, that of a cumbersome pressing apparatus, and the insufficiency of packings; but as the circular plate in this construction was made to remain constantly in the digester, I found that though it was laid near the side of it, in some experiments this mode was attended with an inconvenience which I was desirous of removing. I succeeded by making another circular ring, turned with both sides conical, to be put between the circular plate and the opening of the digester, fitting exactly to each of them. This ring *a b*, *Fig. 5. Plate IX.* passes over the cover *c d*, when both are lowered, the ring remaining in the digester; but the circular plate is taken out through the enlarged opening *e f*. Though

I had

I had by this contrivance,\* in a great measure diminished the inconveniences, yet the intermediate ring remaining in the vessel, and the increased difficulty of executing a more complicated instrument, urged me to attempt some other more expeditious mode of closing.

I should almost be ashamed to confess that the most simple and, as I presume, the best contrivance of all, did not occur to me before I had tried the more complicated ones; if many respectable examples had not proved that the easiest and shortest method is often the most difficult to find. The reason is obvious: Simplicity, like truth, is only one; the bye paths of error are innumerable.

Simple and most accurate closure.

This method consists in making the cover simply an oblong, square, or oval plane brass plate, accurately ground against the inferior surface of a similar frame of the same metal, fixed and soldered to the opening of the digester. This frame has an oblong aperture, through which the cover may be put in or taken out at pleasure. When put in and pressed gently against the frame, it will by the means of a little oil,† have adhesion enough to bear its own weight, till the steam begins to act, which will close it perfectly. But for greater security a small piece of wood, fixed with a screw, will keep the plate joined to the frame, and serve at the same time as a handle. *Fig. 1.* *Plate IX.* represents the longitudinal section of the whole instrument, with all its parts; *ab* is the covering plate, ground and fitted against a square metallic frame *cd*, soldered ‡ to the vessel at *ef*, which ought to be made of hammered copper at least  $1\frac{1}{2}$  line thick. *Fig. 2* is the transverse section of the same apparatus, and *Fig. 3.* a plan of the brass plate, where the dotted lines mark the opening of the frame *cd*, *Fig. 1.* and the

Description of the same. It is a plate which fits against a surface within.

\* The digester with a conical plate, and the safety-piston described below, first executed in Stockholm, 1793, was exhibited to the members of the Royal Society in Berlin, in Feb. 1802; and a description of it, printed in the *Observations Physiques* of Mr. Delametherie, Feb. 1803.

† The different force of adhesion of the same metal, in employing different oils, is remarkable, and may afford a field for future investigation.

‡ Besides soldering, the parts should be joined together with rivets or screws.



part covered by the same. *Fig. 4. hi* is the wooden handle turning round the screw *g*, *Fig. 1, 2.* to connect the metallic planes. It is a kind of button or key.

Peculiar safety valve by which the pressure may be regulated.

My next object was to render the instrument safe, and to place the operator's mind independent of any apprehension of fatal accidents. For this purpose, (besides the strength of the vessel and its different parts, which ought to be as solid as conveniently can be made,) a metallic cylinder *mn* is fixed to the plate *ab*, to the inferior open extremity of which a metallic sphere *mb*, perforated with small holes, adheres by mere friction, allowing the vapour, but not the solid or other matters, to pass into the cylinder, which contains a solid piston *ko*, made of the same metal \* and exactly ground into it, so as to sink down by its own weight if lifted, without however permitting the vapour to escape. This vapour when heated to a certain degree, will raise the piston above one or more of the capillary holes *ss*, on the side of the cylinder and then escape. The piston will remain stationary or oscillating at a height, where the quantity of steam escaping is equal to the quantity produced in the same time by that fire, consequently at a low point if the fire is weak, or, if the fire is intense, at the top, where a larger hole presents a complete issue to the steam and prevents the possibility of accidents.

Conditions for accurate experiment with the digester. 1. To know the temperature. 2. To command the heat, and 3. To render it steady though the fire be not so. The temperature measured by a thermometer.

To adapt the instrument thus constructed to exact and comparative philosophical experiments, three things seem to be required: 1. to ascertain the degree of heat of the interior fluid contents † by a thermometer; 2. to increase that heat at pleasure; and 3. to fix it invariably, if it be desired, independent of the possible increase of the fire, during the course of the experiment.

The first is obtained by means of a small iron cup *p q*, soldered into the plate, and containing some mercury. A fine thermometer passed through a cork, being fixed in the cup, the

\* Great care should be taken to make both of the same metal, for in one instance when I had the piston made of the same brass with the cylinder, but of another day's casting, this infinitely small difference was still sufficient to produce a different force of expansion, in consequence whereof the piston, which moved freely at 212°, ceased to move, and was quite fastened at 260°.

† It is perhaps superfluous to observe that a digester is only intended for experiments with fluids or bodies surrounded with them, and consequently never contains materials in a dry state.

degree



degree of heat is easily observed on the stem. This degree will not be exactly the same with that of the interior space; it shews always somewhat less, which fact was long time ago observed by Mr. *Braun* of the academy of Petersburg; but the difference is small, and being once ascertained, by experiment made on purpose, a due correction, by adding that difference, is easily made.

The heat is communicated in my apparatus by a common Argand's lamp, which was always found sufficient and of which the effect may be regulated by the pinion of that instrument. If time be an object, or if the digester is much more than double the size of *Fig. 1.* the scale of which is one third of the real size, two such lamps may be applied. But a lamp with spirit of wine and three wicks will supply fire enough for a temperature of 270 or 280 if the digester be small and exactly closed. As the atmosphere continually takes away a part of the heat communicated to the vessel, it is only the difference of the communicated and dissipated heat which acts on the steam, and determines the limits of its force, when the fire is weak, and the digester of a large size. On the other side, the heat of the interior fluid and the elasticity of steam corresponding to it, cannot, after having lifted the piston, be increased by a continued fire or by a greater number of lamps, but remains from that moment constant, which seems to be a particular property of this construction. A farther *increase* of heat, as the 2d. desideratum, can only be obtained by opposing a greater resistance to the action of the vapour beneath the surface *k*, which is performed by loading the piston with one or more of the weights *rr*, *Fig. 1.* Thus we may either add new weights progressively, till the thermometer indicates the degree of heat required, or knowing by the former experiments of philosophers the corresponding ratio of increased pressure and heat of elastic fluids, we can load the piston at once with a weight proportional to the intended heat and the surface of its base *k*. This will then rise as soon as that degree is produced, and allowing the steam to escape from one or more of the holes \* prevent

The heat given by a lamp.

The heat is constant if the pressing weight be not changed.

What is the second condition.

\* By what is said above it is evident that the height of the piston, or the number of the holes open to the steam, is of no consequence, it being always compressed by the same weight, and acting with the same elasticity. The *quantity* produced is only different.

any

any further increase of the heat, which will remain *invariable* as the third desideratum required.\*

Very great disadvantage of imperfect closure of the digester.

Here it seems proper once again to insist upon the great importance of a perfect closure in this instrument. If the heated steam, by inaccuracy in the construction, is permitted to escape, not only a great part of the contents is wasted without any use, before the experiment can be said to begin, which sometimes obliges us to open the digester and fill it a second time, but even some of the materials may be altered and decomposed before the intended effect can take place. Still more, very often in such cases, a strong kitchen fire, with the greatest exertions of the operator, (not to mention the trouble and inconvenience of such an operation) is insufficient to afford the degree of heat, which a single lamp, placed with the apparatus on a small writing table, would produce with ease, and without waste of the materials.

In the present state of natural philosophy, the progress of the science seems to depend in a great measure on the perfection of our instruments. In this point of view the preceding attempts to render a little used but very excellent machine more safe, exact, and convenient †, may, if it should not answer that purpose, at least give rise to other more successful improvements.

\* A short description of this contrivance was presented to the *Philosophical Magazine* in June, 1803, and published in December the same year; but by the use of the word *piston* instead of *digester* in a note, an erroneous statement is given in the printed account, which, however trifling it may appear, it is proper to correct, as this part of the instrument has not been changed at all, since it was first made and known in Sweden ten years ago. In the same paper I proposed the use of it for regulating the fire in boilers of steam engines, &c.—and its effect as a *regulator*, both of the *power* of the *steam*, and the *intensity* of the *fire*, depends entirely upon the *weight* being constant and *invariable* in the whole motion of the piston.

† Digesters, as described in this paper, are made by Mr. FIDLER, mathematical instrument maker, 23, Oxford-market, London.

## II.

*Investigation of certain Theorems relating to the Figure of the Earth.* By JOHN PLAYFAIR, F. R. S. Edin. and Professor of Mathematics in the University of Edinburgh.

(Concluded from Page 116.)

21. A REMARK, that is in no danger of being reckoned hypothetical, is, that the conclusion derived from the comparison of degrees of the meridian, with degrees of the circle perpendicular to it, becomes of necessity more liable to error as we advance into higher latitudes. The reason is, that whatever error is committed in determining the magnitude of  $D' - D$ , must be multiplied into the square of the secant of the latitude, in order to give its full effect in changing the value of the fraction  $\frac{c}{a}$ . For it has been shewn that  $\frac{c}{a} = \frac{1}{2} \left( \frac{D' - D}{D'} \right) \sec^2 \phi$ ; now, if we suppose the error committed in ascertaining  $D' - D$  to be in all cases the same, the error of the fraction  $\frac{D' - D}{D'}$  will also be in all cases nearly the same, the denominator  $D'$  being but little affected either by the supposed error, or by the change of latitude. But this error, which may thus be considered as a constant quantity, when multiplied into  $\frac{1}{2} \sec^2 \phi$ , gives the variation or error in  $\frac{c}{a}$ , which error therefore increases, *ceteris paribus*, as the square of the secant of the latitude, so that, on approaching the pole, it increases without limit, and is ultimately infinite. Comparisons of this kind may therefore be expected to give results the more accurate the nearer they are to the equator, under which circle they will be the most accurate of all. Here, again, however, another circumstance must be taken into consideration, viz. that the method of ascertaining the differences of longitude by the convergency of the meridians, so convenient in surveys of this kind, is applicable only in high latitudes. In a trigonometrical survey, therefore, of a country lying much farther south than Britain, a different method of ascertaining the longitudes of places must necessarily be adopted.

This method by the meridian and its perpendicular is more erroneous the higher the latitude.

22. The



To determine the figure of the earth from measures of the perpendicular in different latitudes.

22. The theorems, which were next proposed to be considered, are those that determine the figure of the earth from the measures of degrees of the curve perpendicular to the meridian, in different latitudes. For this purpose let  $D'$  be a degree of one of these curves, in the latitude  $\phi'$ , and  $D''$  a degree of one of them, in another latitude  $\phi''$ . Then  $c$  being the compression, as before, we have by § 18.  $mD' = a + c \sin^2 \phi'$ ,

$$\text{and also} \quad mD'' = a + c \sin^2 \phi''.$$

Hence  $m(D' - D'') = c(\sin^2 \phi' - \sin^2 \phi'')$ , and

$$\text{therefore} \quad c = \frac{m(D' - D'')}{\sin^2 \phi' - \sin^2 \phi''}.$$

This formula may be rendered more convenient for calculation, by considering that  $\sin^2 \phi' = \frac{1 - \cos 2\phi'}{2}$ , so that

$$\sin^2 \phi' - \sin^2 \phi'' = \frac{1 - \cos 2\phi' - 1 + \cos 2\phi''}{2} = \frac{\cos 2\phi'' - \cos 2\phi'}{2}.$$

But  $\cos 2\phi'' - \cos 2\phi' = 2 \sin(\phi' + \phi'') \times \sin(\phi' - \phi'')$ , wherefore  $\sin^2 \phi' - \sin^2 \phi'' = \sin(\phi' + \phi'') \times \sin(\phi' - \phi'')$ , and  $c = \frac{m(D' - D'')}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$

23. In the same manner, because  $mD' = a + c \sin^2 \phi'$ , by substituting for  $c$ , we have

$$mD' = a + \frac{m(D' - D'') \sin^2 \phi'}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}, \text{ and}$$

$$a = mD' - \frac{m(D' - D'') \sin^2 \phi'}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

$$24. \text{ Lastly, since } mD' = a + c \sin^2 \phi',$$

$$\text{and } mD'' = a + c \sin^2 \phi'',$$

dividing the first of these equations by the second, and rejecting the higher powers of  $c$ , we have

$$\frac{D'}{D''} = 1 + \frac{c}{a} (\sin^2 \phi' - \sin^2 \phi''), \text{ and therefore,}$$

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin^2 \phi' - \sin^2 \phi''}. \text{ Hence also}$$

$$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}; \text{ or more conveniently for calculation by logarithms, } \frac{c}{a} = \frac{D' - D''}{D'' \sin(\phi' + \phi'') \times \sin(\phi' - \phi'')}.$$

25. We may compare this value of  $\frac{c}{a}$  with that obtained in § 18. from other data, in order to determine which of the two methods of finding  $\frac{c}{a}$  is to be preferred, under given circumstances. Suppose, for instance, a degree of the curve perpendicular to the meridian, in the latitude  $\phi'$  to be  $D'$ , and a degree of the meridian itself in the same latitude to be  $\Delta'$ ; it is required to find in what other latitude  $\phi''$ , a degree  $D''$ , perpendicular to the meridian, must be measured, in order that the comparison of  $D'$  and  $D''$ , and of  $D'$  and  $\Delta$ , may give values of  $\frac{c}{a}$ , in which the probable error is the same.

To determine the figure of the earth from measures of the perpendicular in different latitudes.

Here, agreeably to an observation already made, we may, in order to estimate the error produced in  $\frac{c}{a}$ , in consequence of an error in the determination of  $D'$ , and  $D''$ , and  $\Delta$ , suppose the error to affect  $D' - D''$ , or  $D' - D''$  only, without paying any regard to the variation of  $D'$  in the denominator. There-

fore, since by § 18 we have  $\frac{c}{a} = \frac{D' - \Delta}{2D' \cos^2 \phi}$ , and again by § 24,

$\frac{c}{a} = \frac{D' - D''}{D'' (\sin^2 \phi' - \sin^2 \phi'')}$ , if we suppose equal errors in de-

termining  $D' - \Delta$ , and  $D' - D''$ , and also that these are the only errors, their effect will be the same, in both cases, if  $2 \cos^2 \phi'^2 = \sin^2 \phi' - \sin^2 \phi''$ . Now, if we suppose  $\phi''$  the quantity sought, and add  $\cos^2 \phi'^2$  to both sides of the preceding equation, then  $3 \cos^2 \phi' = \sin^2 \phi' + \cos^2 \phi' - \sin^2 \phi'' = 1 - \sin^2 \phi'' = \cos^2 \phi''$ . The latitude  $\phi''$  therefore must be such, that  $\cos \phi'' = \sqrt{3} \times \cos \phi'$ . If, therefore,  $\phi'$  be such that  $\cos \phi' =$

$\frac{1}{\sqrt{3}}$ , the cosine of  $\phi''$  will be  $= 1$ , and  $\phi''$  therefore  $= 0$ . Now,

$54^\circ 44'$  is the arch of which the cosine  $= \frac{1}{\sqrt{3}}$  nearly, therefore

if a degree of the meridian, and of the perpendicular to it, be measured in latitude  $54^\circ 44'$ , the comparison of these with one another will give a result as accurate as if the degree of the perpendicular, in that latitude, were compared with the degree at the equator, and more accurate of consequence, than if any other degree of the perpendicular to the meridian, were to be compared with  $D'$ .

26. Hence,

To determine the figure of the earth from measures of the perpendicular in different latitudes.

26. Hence, also, the comparison of the degree of the meridian, and of the perpendicular to it, in the south of England, is better than if a degree of the perpendicular measured in that latitude were compared with a degree at the equator. For if, in the equation  $\cos \phi'' = (\cos \phi') \times \sqrt{3}$ , we make  $\phi' = 50^\circ 41'$ , (or any thing less than  $54^\circ 44'$ ,)  $\phi''$  will come out impossible.

27. It may be shewn, too, nearly in the same manner, that if a degree of the perpendicular to the meridian were measured in Siberia, as far north as the latitude of  $71^\circ$ , supposing that to be possible, and compared with a degree in latitude  $45^\circ$ , or even considerably farther south, it would not give a result so exact as the degree of the meridian and perpendicular measured in the south of England. This shews, that the method of ascertaining the figure of the earth, proposed by the authors of the *Trigonometrical Survey*, (*Phil. Trans.* *ibid.* p. 529) as a subject of future inquiry, is less exact than that which is founded on their own observations.

Whether the comparison of a degree of the merid. and perp. in same lat. be more accurate than that of merid. degs. in different lats.

28. We may also ascertain, by the same means, the relative accuracy of the method of finding the figure of the earth, from the comparison of a degree of the meridian with a degree of the perpendicular in the same latitude, and of the method of resolving the same problem by the comparison of two degrees of the meridian in different latitudes.

If, then,  $D$  be a degree of the meridian, and  $D'$  of the perpendicular, in latitude  $\phi$ , and if  $\Delta$  be a degree of the meridian in a different latitude  $\phi'$ , it is required to find whether the most accurate value of  $\frac{c}{a}$  will be found, by comparing  $D$  and  $D'$ , or  $D$  and  $\Delta$ .

Since we have, by what has been already stated, § 4.

$$mD = a - 2c + 3c \sin^2 \phi, \text{ and}$$

$$m\Delta = a - 2c + 3c \sin^2 \phi', \text{ we have also}$$

$$\frac{D}{\Delta} = 1 + \frac{3c}{a} (\sin^2 \phi - \sin^2 \phi') \text{ and therefore,}$$

$$\frac{c}{a} = \frac{D - \Delta}{3\Delta (\sin^2 \phi - \sin^2 \phi')}.$$

Now, it has been already shewn, that, by comparing  $D$  and  $D'$  we have  $\frac{c}{a} = \frac{D' - D}{2D' \cos^2 \phi}$ . Supposing, therefore, equal errors to be committed in the determination of  $D - \Delta$ , and of  $D'$



$D' - D$ , and also paying no regard to the inequality of  $\Delta$  and  $D'$  in the denominators of these fractions, as it is not so great as materially to affect the quantity that is sought for here, we shall

have the errors in  $\frac{c}{a}$  nearly the same in both formulas, when  $\phi$

and  $\phi'$  are such that  $2 \cos^2 \phi = 3 \sin^2 \phi - 3 \sin^2 \phi'$ , or when

$\frac{2}{3} \cos^2 \phi = \sin^2 \phi - \sin^2 \phi'$ , that is, adding  $\cos^2 \phi$  to both sides,

$\frac{5}{3} \cos^2 \phi = \sin^2 \phi + \cos^2 \phi - \sin^2 \phi'$ , and, therefore,

$\frac{5}{3} \cos^2 \phi = 1 - \sin^2 \phi' = \cos^2 \phi'$ , or  $\cos \phi' = (\cos \phi) \sqrt{\frac{5}{3}}$ .

29. If, therefore,  $\cos \phi = \sqrt{\frac{3}{5}}$ ,  $\cos \phi' = 1$ , that is  $\phi' = 0$ ,

so that  $\Delta$ , the second of the degrees of the meridian, must

in this case be under the equator. But  $\sqrt{\frac{3}{5}}$  is the cosine of

$39^\circ 14'$ , in which latitude therefore if  $D$  and  $D'$  be measured,

the result, by comparing them with one another, is as exact as

if  $D$  were compared with the degree under the equator.

Hence, if  $D$  and  $D'$  are measured in a lower latitude than the above, the result will be more exact, than if  $D$  were compared with the degree at the equator.

If we suppose  $D$  and  $D'$ , measured in the south of England, so that  $\phi = 50^\circ 41'$ ; then we will have  $\phi' = 35^\circ 7'$ , so that  $D$  must be compared with a degree of the meridian as far south as  $35^\circ 7'$ , in order that the result may be as good as when  $D$  and  $D'$  are compared with one another.

From this it is evident, that the method of comparing degrees of the meridian, and perpendicular in the same latitude, has even an advantage over the comparison of degrees of the meridian in different latitudes, unless these last are taken at a considerable distance from one another.

In this way may many useful conclusions be derived concerning the degree of credit due to measurements already made, as well as with respect to the selection of the places where they are to be made hereafter. On these I shall enter no further at present, and shall only add, that, besides the advantages or disadvantages which the method of comparing together degrees of the meridian and perpendicular in the same latitude has, and which are subjects of calculation, it has another

The former method is preferable unless the degrees of the meridian be far asunder.

The former method also supposes the same observers and the same instruments.

other

other advantage, which in the case of the British survey is undoubtedly very great, viz. that all the *data* are furnished from one system of trigonometrical operations; executed according to the same plan, with the same instruments, and by the same observers.

To determine the earth's figure by comparing an arc of the mer. with that of a parallel in same lat.

30. One other application of geometrical measurements to discover the figure of the earth yet remains to be considered. This is the comparison of an arch of the meridian with an arch of a parallel of latitude which crosses it. The measure of a parallel of latitude can be executed readily, and is not confined to a small arch, as in the case of a perpendicular to the meridian. The plumb-line, while it is carried along the circumference of a parallel to the equator, tends continually to the same point in the earth's axis, so that there is no difficulty in ascertaining the amplitude of the arch measured, providing there be no unusual disturbance of the direction of gravity. As an arch of a parallel to the equator, however, is not the shortest line between two points on the surface of the spheroid, the measurement along that surface will not give the length of the arch truly. To obviate this difficulty, it is only necessary to follow the method so properly introduced into the *Trigonometrical Survey*, of reducing the measures, both of lines and angles, to the chords and to the planes of the rectilineal triangles contained by them. In this way, the chord of an arch of a parallel of latitude may be determined, however great the arch; and it is worthy of being remarked, that, whatever be the deflections of the plumb-line at the intermediate stations, when the reductions are all properly made, the length of the chord measured will not be affected by them; the amplitude of the arch indeed may be affected by such deflections, if they happen at its extremities; but the effect of this error will be rendered the less, the greater the arch that is measured. We may suppose, therefore, that the chord of a large arch of a parallel of latitude is measured, and the amplitude of the arch itself at the same time accurately ascertained. This last may be done, either by measuring the convergency of the meridians, if it be in a high latitude, or by any other method of ascertaining differences of longitude which admits of great accuracy. The chord being thus given in fathoms, and the arch subtended by it being given in degrees and minutes, the radius of the parallel itself becomes known,

31. Now,

31. Now, if we would compare the radius of a parallel thus found, with a large arch of the meridian, we shall have by that means a determination of the figure of the earth, not less to be relied on than that given in the beginning of this paper. The investigation is easy by help of the theorems in § 5 and 6. To determine the earth's figure by comparing an arc of the mer. with that of a parallel in same lat.

Let FO be the radius of a parallel to the equator, which passes through F, the latitude of which is  $\phi$ , and is supposed known; and let FO found by the method just described be  $= r$ , then,

$$\text{as in § 4. } r = \frac{a^2 \cos \phi}{\sqrt{a^2 \cos^2 \phi + b^2 \sin^2 \phi}} = \frac{a \cos \phi^2}{a \sqrt{1 - \frac{2c}{a} \sin \phi^2}},$$

according to the method of reduction followed in the preceding articles of this paper. Then, because  $\sqrt{1 - \frac{2c}{a} \sin \phi^2}$

$$= 1 + \frac{c}{a} \sin \phi^2 \text{ nearly, we have } r = a \cos \phi \left( 1 + \frac{c}{a} \sin \phi^2 \right) =$$

$$a \cos \phi + c \sin \phi^2 \cos \phi, \text{ or if we divide by } \cos \phi, \frac{r}{\cos \phi} = a +$$

$$c \sin \phi^2. \text{ Let } \frac{r}{\cos \phi} = l, \text{ then } l = a + c \sin \phi^2.$$

32. Again, if  $\phi'$  and  $\phi''$  are the latitudes of the extremities of an arch of the meridian, the length of which has been measured, and found  $= l'$ , then, according to § 5 we have

$$l' = a(\phi'' - \phi') - \frac{c}{2} \left( (\phi'' - \phi') + \frac{3}{2} (\sin 2\phi'' - \sin 2\phi') \right).$$

If, therefore,  $m$  be the coefficient of  $a$ , in the former equation, and  $n$  the coefficient of  $c$ ; and if  $m'$  be the coefficient of  $a$ , in the latter equation, and  $n'$  of  $c$ , we have, as in § 6.

$$a = \frac{n'l - nl'}{mn' - m'n}, \text{ and } c = \frac{m'l - ml'}{mn' - m'n}, \text{ or since } m = 1,$$

$$a = \frac{n'l - nl'}{n' - m'n}, \text{ and } c = \frac{m'l - l'}{n - m'n}; \text{ also } \frac{c}{a} = \frac{m'l - l'}{n'l - nl'}.$$

33. In this way of determining  $a$  and  $c$ , the parallel of latitude may either intersect the arch of the meridian measured or not. If it intersect that arch, this method may have the same advantage that was taken notice of in another solution, viz. that the whole of the data may be furnished from the same system of trigonometrical operations. Thus, in the survey of Great Britain, an arch of five or six degrees of a parallel to the equator might be measured, and compared with the whole length



To determine the earth's figure by comparing an arc of the mer. with that of a parallel in same lat.

length of the meridian, comprehended between the northern and southern extremities of the island, amounting nearly to nine degrees.

It is plain, from what has already been said, that the result deduced from this comparison would possess every advantage, and would be entitled to more credit, than any determination of the figure of the earth that is yet known.

34. On the supposition that, in a survey of a country, the measurement is made along a series of triangular planes, all given in position and magnitude, there is yet another method of determining the figure of the earth, more general than any of the former. On the supposition just mentioned, it is evident, that the length of a straight line or chord, drawn from a given angle of any one of these triangles, to a given angle of any other of them, may be found by trigonometrical calculation. Let the latitudes be observed at the extremities of this chord, and also the difference of longitude; then, from the nature of an ellipsoid, the length of this same chord may be expressed, in terms of the axes  $a$  and  $b$ , together with the latitudes of the extremities of the chord, and the difference of longitude between them; and this expression being put equal to the length of the chord measured, will give an equation, in which all the quantities are known, except  $a$  and  $b$ . Further, if  $a = b + c$ , and if the said expression be reduced into a series, with the powers of  $c$  ascending, that series will converge very rapidly, because  $c$  is small in respect of  $a$ ; then, for a first approximation, we may reject all the terms that involve the powers of  $c$  higher than the first, by which means we shall have a simple equation of the form  $ma + nc = l$ , where  $m$  and  $n$  are functions of the latitudes and difference of longitude, and  $l$  is the length of the chord.

Now, if a similar equation be derived from the measurement of any other chord, these two equations will give  $a$  and  $c$  in the same manner as in § 6; and thus, from the measurement of any two chords, the figure of the earth will be determined.

35. The length of the chords, thus measured, should be great, so that they may, if possible, subtend angles of several degrees, and their position will be most favourable when one of them is in the plane of the meridian, and the other nearly at right angles to it. The numerical computation will be found less laborious than might be imagined; but the complete

plete solution of the problem, and the full detail of the investigation, I am under the necessity of delaying to some future communication.

To determine the earth's figure, by a general method with triangles.

There seems to be but one difficulty of any consequence that stands in the way of this method of determining the figure of the earth. It arises from this, that the ascertaining the position of the supposed series of triangular planes relatively to one another, involves in it the allowance to be made for the terrestrial refraction, which it must be confessed is not accurately known, and is the more difficult to determine, that it is unavoidably combined with the irregularities in the direction of gravity. It is possible, indeed, to separate these two sources of error, but not without a system of experiments instituted directly for that purpose.

36. The determination of the difference of longitude, which enters necessarily into this problem, except in the case when both chords are in the direction of the meridian, must also be performed with great accuracy. Among the different ways of doing this, that which proceeds by observing the convergency of the meridians, though the best accommodated to the nature of a trigonometrical survey, is not the least liable to objection. For, not to mention that it is only practicable in high latitudes, we must observe, that it always implies a correction on account of the ellipticity of the meridian, which is therefore necessarily hypothetical, and depends on the very thing that is to be found. This inconvenience, however, may be obviated by repeated approximations, and by an accurate solution of *spheroidal* triangles. On this latter subject it was my intention to offer to the Society some theorems, that contain more direct and fuller rules for this kind of trigonometry than any that I have yet met with. I am under the necessity, however, of reserving these, as well as the solution of the problem above-mentioned, for the subjects of some future communication. In the mean time, I think it is material to observe, that the principle laid down by Mr. Dalby, viz. that in a spheroidal triangle, of which the angle at the pole and the two sides are given, the sum of the angles at the base is the same as in a spherical triangle, having the same sides, and the same vertical angle, is not strictly true, unless the eccentricity of the spheroid be infinitely small, or the triangle be very nearly isosceles. The application of the principle may therefore lead into error, unless it be made with due

attention to these restrictions. The gentleman, just named, will forgive a remark, which I certainly should not have made, if I had been less interested for the success of the work, in which he has assisted with so much ability.

### III.

*Enquiries concerning the Nature of a Metallic Substance lately sold in London as a new Metal, under the Title of Palladium.*

By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A.\*

(Concluded from page 101.)

#### EXPERIMENTS TO PROVE AFFINITY AMONG THE METALS.

Instances of affinity in metals silver precip. by muriate of platina carried down 21 per. cent. of the metal.

*Exp. 1.* I dissolved one hundred grains of silver in nitric acid, and precipitated by neutral muriate of platina. The precipitate, well washed and dried, was of a bright straw-colour, and weighed 147 grs. Reduced in a charcoal crucible, it yielded a button weighing 121 grs. and of the specific gravity of 11,6. The difference of weight, between the original hundred grains of silver and the 121, was owing to 21 grains of platina, which had been drawn down in precipitation along with the silver; by an affinity for that metal. This alloy is acted upon by nitric acid, and a great part of the platina is dissolved along with the silver, nor is it very easy to separate them by the common methods.

Mercury and silver precipitated in amalgam by green sulphate.

*Exp. 2.* I dissolved one hundred grains of silver in nitric acid, and added about 1200 of mercury. I poured the mixed solution into a solution of green sulphate of iron, and obtained a very copious precipitate. When washed and dried, it weighed 939, and was a perfect amalgam, in the due proportion of mutual saturation. Its specific gravity was 13,2; but no mercury remained with it after exposure to heat.

Mercury and gold precipitated by green sulphate gave a fine blue metallic powder.

*Exp. 3.* I dissolved one hundred grains of gold in nitromuriatic acid, and added to it about 1200 grains of mercury. Green sulphate of iron, poured into this mixed solution, caused a precipitate weighing 874. It was in the form of a fine blue powder, not resembling an amalgam, though wholly metallic. Its specific gravity I could not ascertain; but all the mercury was expelled by heat.

Experiments with recent muriate of tin, and

The re-agents which I used in the following experiments, were recent muriate of tin, and green sulphate of iron. To bring



bring the examples of anomalous precipitations, in mixed solutions of the metals, more clearly into view, it will be necessary to state the action of these salts, upon a solution of each metal when separate.

By recent muriate of tin we have, with a solution of gold, the well known purple of Cassius. With platina, the colour of the liquor is much heightened. With mercury, there is a total reduction. With copper, a reduction from the black oxide at 20 per cent. of oxygen, to the yellow oxide at 11,5 per cent. of oxygen. With arsenic acid, a reduction to the state of white oxide. With silver, with lead, with antimony, no reduction. Green sulphate of iron reduces none of the metallic solutions, except those of gold and silver.

green sulphate of iron, as reagents.

Action of the recent muriate of tin on solutions of gold, platina, mercury, copper, arsenic acid, silver, lead, antimony.

of green sulphate of iron, reduction of gold and silver only.

When mixed solutions of the metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, we have the following results.

*Experiments 4, 5, 6, 7, and 8.* Muriate of tin, poured into a mixed solution of gold and mercury, precipitates both metals together; and there are no traces of the purple. Mixed solutions of gold and antimony, also of gold and arsenic acid, are acted upon in the same manner. Mixed solutions of gold and copper, also of gold and lead, afford results similar to those of each metal when separate.

Exp. with mixed solutions. Muriate of tin the reagent.

*Experiments 9, 10, 11, 12, and 13.* With a solution of platina and arsenic acid, muriate of tin gives no precipitate; but the colour of the liquor is more heightened than if the platina had been alone in solution. Platina and antimony give a precipitate by this re-agent, after standing some time; but the effect is retarded by the excess of acid in the solution of antimony. Platina and copper, also platina and lead, are acted upon as the separate solutions of these metals. Platina and silver are precipitated together by green sulphate of iron.

Green sulphate

*Experiments 14, 15, 16.* Mercury and copper, mercury and lead, also mercury and arsenic, are precipitated in the metallic state by recent muriate of tin.

From these experiments it is evident,

1st. That gold has an affinity for mercury, for antimony, and for arsenic.

Deductions of affinity between gold and mercury, antimony, arsenic; platina and silver, mercury, antimony; silver and mercury; mercury and copper, lead, arsenic.

2d. That platina has an affinity for silver, for mercury, and for antimony; and that it is influenced by the presence of arsenic.

3d. That silver has an affinity for mercury.

4th. That mercury has an affinity for copper, for lead, and for arsenic.

This series of experiments is not intended as a system of metallic affinities; but as a few facts stated to corroborate an assertion. I am well aware that many others might be noticed; but it is not my intention to enter further into this subject, in the present paper. The general importance of the principle, and the extensive influence it is likely to have upon chemistry, demand that it should be treated by multiplied researches. The experiments that can elucidate it are of the most delicate nature, and require peculiar care; for they do not always succeed, unless performed under the most favourable circumstances.

Mixed solutions of more than two metals are more striking and more complex.

When mixed solutions of three or more metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, their action upon each other appears in a much more striking, as also in a much more complicated point of view.

#### EXPERIMENTS UPON PLATINA.

Exp. on platina.

I shall now state some experiments which I have had occasion to make upon platina, during the foregoing researches. Very little is known concerning this metal, its oxides, or its salts; and, although I have not had occasion to extend the enquiries very far, yet my experiments may serve to establish a few points.

Platina dissolved precip. by lime; redissolved in nitric acid; the acid expelled from the oxide by heat; oxide reduced by simple ignition.

I dissolved a quantity of purified platina \* in nitro-muriatic acid, and precipitated by lime. A great portion of platina remained in the liquor, although I had used an excess of the above earth. I redissolved the precipitate in nitric acid, and evaporated to dryness. The result was, a subnitrate of platina. I then exposed the mass, in a crucible, to a heat capable of expelling the acid altogether; and the oxide remained alone. When this was reddened, at a heat which certainly was not capable of melting silver, the oxide was reduced, and appeared with a metallic lustre. The weight of the various

\* By purified platina, I have always understood, in this Paper, platina reduced, at a gentle heat, from the salt obtained by pouring a concentrate solution of muriate of ammonia into a concentrate solution of platina.

products.

products, in the above experiments, was such as to give the following proportions in the oxide, and the subnitrate of platina.

Yellow oxide of platina is composed of,

Platina	-	-	-	87
Oxygen	-	-	-	13
				<hr/>
				100.

Hence component parts of the oxide;

Subnitrate of platina is composed of,

The above oxide of platina	-	89
Nitric acid and water	-	11
		<hr/>
		100.

and of the sub-nitrate.

But, in the reduction of this oxide of platina, it became of a green colour; and remained during some time in that state. Nitrate of platina sometimes becomes of a pale green at the edges, when evaporated to dryness; and ammonia assumes a green colour when it holds oxide of platina in solution, as we have seen more particularly with palladium. This, therefore, is a second oxide of platina. It contains but seven per cent. of oxygen.

Two oxides of platina; yellow with 13 per. cent. oxygen; green with only 7 per. cent.

I dissolved a known portion of platina in nitro-muriatic acid, and expelled the nitric acid, by pouring in a sufficient quantity of the muriatic; and then evaporated to dryness. By this experiment I learned, that the insoluble muriate of platina is composed of,

Yellow oxide of platina	-	-	70
Muriatic acid and water	-	-	30
			<hr/>
			100.

Component parts of insoluble muriate of platina;

I then expelled the muriatic acid by the sulphuric, and evaporated again to dryness. I found the insoluble sulphate of platina to be composed of,

Oxide of platina	-	-	54,5
Acid and water	-	-	45,5
			<hr/>
			100,0.

and of the insoluble sulphate.

By much the most delicate test for platina is muriate of tin. A solution of the former, so pale as hardly to be distinguished from water, assumes a bright red by a single drop of the recent muriatic solution of the latter metal. If mercury be present, the colour is much darker. Recent muriate of tin, poured into a solution of the muriate formed by the red oxide of mercury,

Muriate of tin is the most delicate test of platina.



converts it into the muriate formed by the less oxygenized acids; but, shortly after, the mercury is reduced to the metallic state. Hence it was, that the alloy of platina and mercury always gave a deeper coloured precipitate than platina, with muriate of tin.

The prussiates do not precip. platina or mercury singly; but they do jointly.

Neither platina nor mercury are precipitated by prussic acid or by the prussiates. But, if sulphate, nitrate, or muriate of platina be poured into prussiate of mercury, an orange-coloured precipitate is immediately formed; and, in some cases, a mixed solution of platina and mercury gives a similar precipitate by prussic acid alone.

Platina is simply precip. by sulph. hydrogen.

Order of affinities of platina with acids.

Platina is one of the metals which are precipitated by sulphuretted hydrogen, without the necessity of a double affinity.

The affinities of platina differ much from what is generally stated in the tables. By the few acids I have had occasion to try, oxide of platina is attracted in the following order: sulphuric, oxalic, muriatic, phosphoric, fluoric, arsenic, tartaric, citric, benzoic, nitric, acetic, and boracic.

Remarkable theoretical observation.

That sulphuric acid should attract the oxide of platina with greater force than the muriatic, is an unanswerable argument to an opinion which was long supported by many philosophers, and which is not yet altogether abandoned by them. Muriatic acid has been said to contribute to the solution of gold or platina, in nitro-muriatic acid, in the same manner as sulphuric acid is supposed to promote the decomposition of water, during the solution of iron by that acid diluted. The affinity of muriatic acid for the oxide of gold or of platina, has been looked upon as the disposing cause that nitric acid is decomposed by those metals. But it is evident that some other action takes place; for, sulphuric acid, which has a stronger affinity for oxide of platina than muriatic acid, does not in the least promote the decomposition of nitric acid by gold, or by platina.

#### CONCLUSION.

On the nature and use of general hypotheses.

The substance which has been treated of in this paper, must convince us how dangerous it is to form a theory before we are provided with a sufficient number of facts, or to substitute the results of a few observations, for the general laws of nature. If a theory is sometimes useful, as a standard to which we may refer our knowledge, it is at other times prejudicial, by creating

ing an attachment in our minds to preconceived ideas, which have been admitted without inquiring whether from truth or from convenience. We easily correct our judgment as to facts; and the evidence of experiment is equally convincing to all persons. But theories, not admitting of mathematical demonstration, and being but the interpretation of a series of facts, are the creatures of opinion, and are governed by the various impressions made upon every individual. Nature laughs at our speculations; and though from time to time we receive such warnings as should awaken us to a due sense of our limited knowledge, we are presented with an ample compensation, in the extension of our views, and a nearer approach to immutable truth.

The affinities of metals for each other are likely to be of the most extensive influence in chemistry. They will promote scepticism with regard to future discoveries, and throw some doubts upon our present knowledge. Palladium is certainly not less different from the elements that compose it, and from all other metals, than any two can be from each other. Within the last fifteen or twenty years, several new metals and new earths have been made known to the world. The names that support these discoveries are respectable, and the experiments decisive. If we do not give our assent to them, no single proposition in chemistry can for a moment stand. But, whether all these are really simple substances, or compounds not yet resolved into their elements, is what the authors themselves cannot positively assert; nor would it in the least diminish the merit of their observations, if future experiments should prove them to have been mistaken, as to the simplicity of those substances. This remark should not be confined to later discoveries; it may as justly be applied to those earths and metals with which we have been long acquainted.

The affinity of metals for each other, so strongly shewn in the present instance, may induce doubts to the nature of other new metals;

With regard to the metals, we have seen how little dependence is to be placed on specific gravities. A contrary anomaly to that which operates upon platina and mercury, may take place in others; and they may become as much heavier than the mean, as the former become lighter. In this state of union, they may for a long time appear homogeneous, even by the test of chemical re-agents. One of the properties that renders metallic substances so precious is, their easy formation into such instruments as our necessities require. The fragile metals are but of secondary consequence; and, at most, serve to conf

Brittle metals may perhaps be still more simplified.



on those which are ductile, some quality which adapts them better to particular purposes. It often happens that, by being alloyed, two ductile metals become fragile; but we have no instance of the contrary effect in any high degree. It is therefore more to be supposed that we should look to simplification among the fragile metals; and, even at this early period, it may not be too speculative to consider the metallic bodies in an order which may bring together those which possess the greatest number of similar characters.

Comparisons of  
metallic bodies  
with this view;

As an instance of this approximation, it may be observed, that nickel and cobalt strongly participate in the properties of copper and iron. The two former metals were long regarded as mixtures; and the doubts of the ancient chemists, who feared to pronounce as to their nature, may still be proved to have more foundation in truth than the assertion of the moderns, who have declared them to be simple. Acted upon by the same menstrua, forming insoluble compounds with the same acids, and soluble alike in other substances, they have but one or two marked properties that lead us to consider them as distinct metals. But palladium has at least five or six characters, as strong as those of any metal whatsoever, that distinguish it, not only from its elements, but also from all other metals.

and earths also.

Among the earths, this approximation is still more apparent. A leading character of these substances is, their tendency to enter into saline combination, in which they receive new properties, and perform new functions. If we rank them according to this general tendency, we shall have the following order: barytes and strontia; lime and magnesia; glucine and alumina; zircon and silica. And, if we consider them two by two in this order, which is a natural one, we shall bring together precisely those which differ by the smallest number of chemical characters.

Conclusion.

This investigation might be pursued still further; but we must wait the result of experiments: a wide field is open for research. In the dark ages of chemistry, the object was, to rival nature; and the substance which the adepts of those days were busied to create, was universally allowed to be simple. In a more enlightened period, we have extended our enquiries, and multiplied the number of the elements. The last task will be to simplify; and, by a closer observation of nature, to learn from what a small store of primitive materials, all that we behold and wonder at was created.

IV. Observation



## IV.

*Observations on some remarkable Strata of Flint in a Chalk-Pit in the Isle of Wight, in a Letter from Sir HENRY CHARLES ENGLEFIELD, Bart. F. R. S. to JOHN LATHAM, M. D. F. R. S. and L. S. \**

DEAR SIR,

AS you considered the specimens of flint which I shewed you worthy of the notice of the Linnean Society, I transmit them to you, together with such an account of the situation in which I found them, as may perhaps lead to a guess of the causes of their present very extraordinary condition, and will at least serve as a guide to those who may wish at a future time to inspect the curious pit where I found them. Introductory observations.

Before I enter on the particular description of that spot, I cannot help saying a few words on the lithology of the island in general, which has not that I know of, been described, as it highly deserves, by any naturalist. Had I been equal to such a task, opportunities of observation were wanting, and the phenomenon which I am about to describe, was discovered by me so short a time before I quitted the island, that I had not time to inspect more than one pit, besides that in which I observed it.

The Isle of Wight, which is nearly of a rhomboidal form, lies with respect to its four angles, almost absolutely in the four points of the compass. It is divided into two very nearly equal parts by a range of chalk hills, whose general direction is due east and west. These hills do not, however, lie in a straight line, nor are they at all of equal breadth or height throughout their extent. At Bembridge, where they form the eastern point of the island, they rise abruptly from the sea to the height of about 400 feet; and bending a little to the northward, they continue of nearly the same elevation, and a very narrow breadth, till they terminate at the valley through which the Medina runs. To the west of the Medina the range grows considerably wider and is subdivided into several subordinate vallies. This additional breadth gives the southern limit a great curvature to the south, while the northern line remains nearly straight. Their elevation increases much, and at Mottistoun is 700 feet. The acute and perpendicular Geological account of the Isle of Wight.

\* From the Transf. of the Linnean Society, vol. IV.

Geological account of the Isle of Wight.

promontory in which they terminate to the west, well known by the name of the Needles is nearly as high as Mottistoun. Beside the valley of the Medina, this range is singularly interrupted by two vallies exactly similar to each other, at the two ends of the island. Brading Haven renders Yaverland at the east almost an isle, and the Yarmouth inlet cuts off the western end so nearly, that at high tides it is quite insulated at freshwater gate.

To the north of this range of chalk hills the soil is chiefly clay, with a superstratum, in many parts of gravel, the clay is intersected with many beds of stone of different qualities, and which appear to lie in great confusion. Of these some are grit with a slight admixture of calcareous matter, others have nearly equal parts of sand and lime, and others are purely calcareous. In the first which are of great hardness, very few extraneous bodies appear. In the second, are many fine impressions of shells, while the last are almost entirely composed of moulds of turbinated shells, so as to appear quite honey-combed by them. This stone is however, of great durability for the walls of Cowes Castle, which was built by Henry VIII. and is exposed to the sea air from the west and north, are as perfect as on the day in which they were built. Below all these strata of stone at east Cowes, and just above a bed of black and solid clay is a stratum of shells about two feet thick, of which a specimen accompanies this; and which is totally composed of these shells without any admixture or earth whatsoever. As the Lea makes great inroads here, vast heaps of these shells lie on the beach, and seem just washed up by the waves, instead of being torn from their bed in the cliff. They appear nearly in the same state as those on the Hampshire coast, which have long been famous among naturalists. In the bed at east Cowes, there appears, however, no variety; for I could see no species but what are here exhibited.

Whatever confusion in the strata appears to the north of the chalk range, or in that range itself, disappears to the south of it, where the strata are nearly in a horizontal position, and singularly regular and undisturbed. The sea coast from Bembridge, south to the Needles, except in the small extent of Sandown Marsh, is every where higher than the immediately contiguous land of the island, and to the south-east, rises into



a vast range of hills running from Dunnose west to St. Catharines. The substratum of these hills seem every where to be clay lying in strata of different colour and purity. The lowest is black and very hard; approaching to shale. Above this some strata have a great mixture of sand, and take the appearance of a soft stone, breaking into very regular cubical forms. These strata extend over the whole southern part of the island, and terminate against the chalk range very suddenly. Above the clay strata is a bed of stone in thin layers, and of very mingled materials, but in general very hard. Great quantities of chert or flint modules appear in this stone. The general thickness of the stratum is from 150 to 200 feet. Above this the highest hills of the range have a stratum of chalk, not pure or white as that of the chalk range properly so called, nor producing flint so black.

Geological account of the Isle of Wight,

The height of Dunnose is 800 feet above low water mark, St. Catharine's hill is at least 850. Of the former I had no opportunity of examining accurately the thickness of the strata, but at St. Catharine's the strata are as follow,

Chalk	-	250 feet
Stone	-	200 feet, or perhaps not quite so much
Clay and Sand	-	400 feet.

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850.

This arrangement accounts entirely for the formation of that singular coast called the Undercliff, which extends from Dunnose to St. Catharine's; and is composed of the confused fragments of the upper stratum of rock, which have given way and rolled down, as the substratum of clay has been washed away by the sea. In most parts the process seems nearly at a stand; the coast being now protected by the fallen rocks; but at St. Catharine's, great devastation is still taking place. The earth-fall mentioned last year, was a very small operation, when compared with the relics of former convulsions.

From this short sketch of the general position of the strata in the island, I return to the particular subject of the present paper.

The chalk pit, which I am about to describe is situated on the northern edge of the chalk range just out of the village of Carisbrook, and about an hundred yards beyond the division of the roads to Yarmouth and Shorwell. The pit is open to the

Description of a chalk pit at Carisbrook, with seams of flint, singularly broken or shattered,



the east. The strata of chalk are very regular from two to five feet in thickness, and divided by seams of flint from six inches to nine inches in depth: The flints are as usual in nodules of different sizes from the size of the fist to twice the size of a man's head. The whole dip northward with an inclination of at least 67 degrees. Perpendicular fissures run through the whole from north to south, the sides of which are nearly as flat and smooth as a wall. As these fissures are followed with convenience in working the pit, an extensive face was laid open when I saw it, and the appearance was as in the annexed sketch. See *Plate XI.* On examining the beds of flint nearly, I was astonished to find that every flint, though lying in its place, and retaining perfectly its original shape, was more or less burst and shattered, some few were only split into large pieces, but the greater part were broken into small fragments, and some absolutely reduced to impalpable powder. From one which had suffered the most, the annexed specimen was taken. The powder was so very fine that I conceived it must have been mixed with chalk; but on washing it with diluted marine acid, I found that it was purely siliceous. Indeed the chalk which surrounds these flints is uncommonly solid, and does not exhibit cracks or marks of any violence except the great fissures before mentioned. A specimen of the flint powder after washing in the acid is sent with the other.

I must observe that I had but an imperfect opportunity of inspecting the flints which lay at a distance from the fissure, such however as I could see in the bed then working appeared to have been less shattered in proportion as they were more remote from the fissure, but all had suffered more or less.

About 200 yards below the pit, and nearer to Carisbrook village, the road is in part cut through the chalk, and the beds of flint exposed by that means exhibit the same appearances as those in the pit above.

Observations of  
the flints in  
another pit.

The chalk pit above Shide Bridge, which is the only one I had an opportunity of examining after my discovery of the phenomenon above described, presents in some degree the same appearances, but does not afford so good an opportunity of viewing the strata as that at Carisbrook. The strata did not appear to me to lie so regularly nor the flints to be disposed so much in beds as at Carisbrook. They were however extremely

tremely broken and shattered and apparently the most so where they lay most in strata. The strata had also a great inclination or dip to the north.

Although it would be rash to attempt to account for this very singular state of destruction of the flints in the Carisbrook pit, yet it is impossible not to offer some conjectures on the subject. There can be very little doubt that the strata though now so inclined was originally formed in a horizontal position. When the tremendous convulsion took place which sunk them to the situation in which they now appear (at which time the channel which separates the Isle of Wight from the main land was perhaps formed) the strata of chalk, in the act of subsidence had a tendency to slide on each other, and this would be exerted most sensibly where from the admixture of the flints the cohesion of the parts of the chalk was the weakest. This motion or rather strain of so enormous a weight might in an instant shiver the flints, though their resistance stopped the incipient motion, for the flints though crushed to powder are not displaced, which must have been the case had the beds slipped sensibly. This conjecture is perhaps strengthened by what I observed in a few detached nodules of flint in the chalk strata which did not appear to have suffered as those in the beds of flint have done. I may here add that it seemed as if in some places the fine powder of the flints had run down, and invested the nearer parts of the fissure with a thin coating of the agglutinated dust; but this may possibly have taken place since the face of the fissure has been exposed to the weather.

Perhaps it may not be totally foreign from the present subject to mention that in a very great chalk pit at the village of Preston, a mile north of Brighthelmstone, in which the flints lie in a very regular and nearly horizontal strata, but which has also vast perpendicular fissures in the chalk; the fissures are in many places filled to a considerable extent with a very thin vein of pure flint exactly as if the flint, not being quite hard when the fissures took place, had been squeezed out of the beds and run into the fissures as soft pitch would do. I do not mean at all to say that this was the case, but merely to describe the appearances. In the chalk pit just below the church at Brighthelmstone another singular appearance may be seen. The upper part of the chalk is in separate masses, not properly rubble, but with all their tender angles sharp exactly as if just broken

Conjecture as to the cause; viz. that the strata at the period when their dip took place were inclined to slide on each other, and the flints were crushed by this enormous action.

Appearances of a chalk pit near Brighthelmstone.



broken to pieces to put into the lime kiln, and quite clean, nearly of a size, and almost without any chalk powder mixed with them.

I remain, &c.

Southampton, Jan. 22, 1800.

## V.

*Description of a mercurial Air-pump, of unlimited exhausting Power, with a Wooden Piston, working in a Wooden Cylinder, &c. By SIR A. N. EDELCRANTZ, &c. &c. Communicated by the Author.*

Construction of an air pump in which no residual space is left.

HAVING resided a long time in a place, where no mathematical instrument maker could be had, I was obliged to contrive some method of constructing an air-pump, in which common workmen and common materials could be employed. Besides which, as in almost all former constructions of this instrument, a small space exists in some part of them, where the air remains condensed, and consequently a limit is fixed to the expansion, I was desirous, if possible, to remove this seemingly material, though in good pumps infinitely small defect. These considerations led me many years ago to a contrivance, of which the following short description may give a notion, reserving a more detailed one, with a more particular account of its effects, to a future publication.

Description with a drawing. It operates by mercury raised and lowered in a wooden pipe, by a plug, and the upper communication with the receiver is made through an iron cock.

A B C D, Plate XII. is a solid piston of wood, cylindrical or square, \* moving up and down in a similar wooden box or tube, E F G H, by means of rack-work at H. The piston fills the box in its whole length as exactly as possible, without touching and producing friction against the sides. For that purpose the piston is guided at the lower end by an iron rod K I, fixed in the bottom of the box and entering the piston, and at its upper part by a roller, which runs along the inner surface of the barrel. The cavity of the wooden box extends through the canal F M † to the iron cock O, and communicates at M with a per-

\* I have, in the execution, preferred the square form, as more easy to make exact and equal in its whole length.

† All the wooden parts are made of strong oak, and are covered with a good resinous varnish, to prevent the absorption of mercury: perpendicular



pendicular glass tube M N and the glass sphere P. This sphere is connected with an iron cock Q R, having two opposite perforations, one going horizontally to R, where a small outside valve permits a communication with the glass ball S Y; the other, placed perpendicularly, opens a passage from the common receiver V T to the sphere. The instrument being thus constructed, the box H E F G, is filled with mercury to the dotted line *aa*, or about six inches, and the piston being alternately pressed down or raised as usual by the crank work, the effect of the machine is as follows:

Air pump operating by mercury in a wooden barrel, &c.

The cock Q R being turned as in the figure, the communication with the receiver is shut, and that with the external air, through a small hole S in the glass sphere S Y, being open, the piston in descending presses upon the surface *aa*, and forces the mercury out of its place, it consequently rises in the small space between the piston and the wooden tube, *aa* to *bb*, and at the same time in the glass tube N P to a corresponding height N. The pressure being continued, the mercury still rises so as to fill the whole tube, the sphere P, and last of all makes its way through the cock Q R and the valve R, forcing every particle of the air contained in M N P out before it. When a drop of mercury appears in Y, the piston is moved the contrary way, and by its ascension the pressure on the mercury is diminished, which sinks and evacuates the sphere P, leaving a place for the air from the receiver which is admitted by turning the cock Q R, in the other direction\*. P being filled with air, the cock is turned again, the piston again descends, and expels the air, by raising the mercury a second time, and in this manner the exhaustion may be continued at pleasure: The mechanism and effect of this instrument being, as I presume, rendered clear by the preceding description, I shall only add a few remarks.

The necessary force to press down the piston, being a little more than the whole weight of a column of mercury of the basis D C, and the height of the barometer, is considerable in the beginning of the operation, but diminishes as in other air-pumps, when the air in the receiver becomes more rarefied; when the external atmosphere acts more powerfully in support

\* The alternate turnings of the cock may easily be performed, by a simple appendage connected with the motion of the piston.

Air pump operating by mercury in a wooden barrel, &c.

of the column M N, which in every successive stroke becomes higher, till the exhaustion is nearly compleat, when its height will be that of the barometer.

In order to equalize the pressing force more nearly during the whole operation, the upper end of the piston may be loaded with a weight equal to half the original pressure.

The mercury being a fluid, fills every angular space as it passes, till it opens its way through the valve R; consequently no particle of air can be left in a condensed state to be more rarified or diminished by the next stroke of the piston. By this means the common defect of air-pumps seems to be remedied, and the exhaustion unlimited. Besides, as neither moisture or oil are introduced in this pump, the vapours arising from them in great exhaustions are prevented.

The quantity of mercury required, when the diameter of the wooden box and of the sphere P are four inches, will be about sixty pounds, which if taken out by the cock O, may serve also for another useful apparatus in the same laboratory: the mercurial air holder.

Several projects for mercurial air-pumps have been published before, although I think none executed; but a comparison will, as I suppose, easily shew this construction to be quite different from any of them.

For greater solidity, I have in the execution made three-fourths of the tube M P of iron. The sphere P is likewise fixed and fastened by means not shewn in the figure, but which may without difficulty be apprehended.

Excepting the cock Q R, which requires some precision of workmanship, all the parts of the machine may be executed by common workmen, such as carpenters, smiths, &c. who are every where to be found.

## VI.

*On the Combinations for supplying Worm Tubes with Cold Water, by the Syphon, with an easy Method of securing the Joints of such Hydraulic Apparatus against the Admission of the External Air. In a Letter from Mr. WM. CLOSE.*

To Mr. NICHOLSON.

SIR,

*Dalton, Feb. 13, 1804.*

THE combinations which have lately appeared in your Journal, for supplying worm tubs, condensers, &c. with cold water, have recalled my attention to certain experiments which I formerly made with the syphon; and I am of opinion that the method proposed will succeed very well upon the simple construction of the apparatus delineated in *Plate IV. Vol. VI.* of your Journal, if all the parts are made air-tight, and a sufficient quantity of water be provided to keep the syphon in constant action, under an impending column of two feet. For the air disengaged from the water in the worm-tub will rise into the bend of the syphon at B. *Plate IV. Vol. VI.* and be carried down the descending column and emitted at C.

Reasons why the syphon principle is applicable to the supply of worm tubs, &c.

The worm tub may be effectually secured against the admission of the external air through the joinings of the staves, by being placed in a larger tub filled with water; no air then can possibly enter the interior tub so long as it is covered, and the workmanship must be very bad, if the syphon should draw the water out of the exterior vessel, so fast as to produce any inconvenience in replenishing it as often as it subsides.

Easy method of excluding the external air, by surrounding the worm tub with water.

The worm must be made air-tight before it is fixed in its place. If the supply of cold water at A should be interrupted, the fluid in A will subside to a level with the surface of C, and, if much air be disengaged during this suspension, and while the water is impending in the apparatus, the syphon will probably not resume its operation: In this case the two apertures must be closed, and the apparatus filled with water.

To set the machine at work when stopped.

You will remember the principles on which I planned the construction of a machine for raising water by the syphon. I have the complete model of such a machine: it has four light, self-acting valves, and when these are not prevented from closing, by impurities in the water, &c. it performs very well.

Account of the effect of a syphon engine formerly described.

The



The syphon is not quite two inches wide; it rises about 34 inches above the reservoir in which it is placed, and the descending column is six inches longer than the ascending one. The machine is constructed with all the parts represented in *Plate III. Vol. I.* but under a different arrangement. It delivers about three pints of water every minute, at 31 inches above the reservoir: the alternations are repeated every five seconds.

Its power in carrying down air.

In the operation of this machine, a bulk of air equal to three pints of water is carried down the descending column every minute, when the fall is only six inches; but I am certain from experiment that the syphon would clear itself of a much greater bulk in the same time.

Hence the worm tub syphon will be effectual in carrying off the air extricated from the water.

In an experiment on the 29th of May 1802, with a syphon of the above dimensions, and a fall of six inches, it appeared by a graduated bottle which received the raised water, that the syphon would take a quantity of air equal in bulk to four ounces of water at least, every three seconds. I think, therefore, that the small quantity of air which will be disengaged from the water in the worm tub at a low temperature, by the operation of the syphon, with a fall of two or three feet, will be carried off as fast as it is produced, unless the pipe be very narrow in proportion to the tub,

I am, Sir,

Your's respectfully,

WILLIAM CLOSE.

## VII.

*An Answer to Dr. WOLLASTON'S Statement, "Of an Improvement in the Form of Spectacle-Glasses." By Mr. WILLIAM JONES, F. Am. P. S. Optician, Holborn.*

The spectacle glasses of Dr. Woollaston censured.

OBSERVING in your Journal for last Month, that Dr. W. H. Wollaston, by a paper inserted therein, is attempting to introduce into the construction of spectacles, the well known and obsolete form of lens called *a meniscus*; I beg leave, Sir, to offer through the channel of your scientific and impartial record, a few observations on his arguments; and my reasons why I do not consider the contrivance as entitled to any claim either of novelty or improvement.

When

When a printed book, or other object, is received through a convex spectacle-glass, or other lens of a short focal distance, such as from seven inches down to four inches or less, the indistinctness observed of the surrounding parts, when the central appear distinct, arises from the spherical figure of the lens, and is by Opticians called the longitudinal aberration of the lens. There is another kind of aberration connected with this lens, called the lateral aberration, which is occasioned by the prismatic form of the lens, producing different refrangibility of the rays of light, and blending the prismatic colours with the appearance of the object. It is the longitudinal aberration only that I have now occasion to consider. This aberration in lenses of the same foci, increases with the diameter and thickness; and of the same diameter is in the inverse proportion to the foci.

Aberration of  
light refracted  
by lenses.

The rays issuing from distant objects, are more parallel to each other when incident upon the lens, than those from proximate ones; therefore the aberration will be less.

Hence, it may be inferred, that when spectacle-glasses have been made larger in diameter than is sufficient to observe through, the angular extent of objects have very properly been somewhat reduced in diameter, so that a person might use them without much inclining the axis of his eyes, or finding it inconvenient or unnatural to move his head a little. For the aberration is thus diminished, and consequently the objection in a great degree removed, except when the glasses are of very short foci.

Contracting the  
diameter of  
spectacle lenses  
approved.

In concave glasses, the aberration or indistinctness is of a similar nature; the defect of these being from the imperfect divergence of the rays, instead of the imperfect convergence in convex glasses.

Spectacle-glasses are now generally made of the double concave, and double convex forms or nearly so; for a little alteration of figure does not affect the general appearance of objects viewed through them. It is in science, as in other cases, that general utility does not always depend on trifling alterations. Spectacles are recorded to have been invented about the year 1300, and from much reading and many years experience in this small, but invaluable article, I really do not know, that during the elapsed time, any optical instrument of any kind whatsoever has undergone more innovation, and

Remarks on  
spectacles and  
some of the im-  
provements  
made in them.

attempts at improvement. Of many within my knowledge, I shall only select the following as entitled to any degree of commendation. *Ayscough's* crown glass spectacles. The bifected glasses of *Dr. Franklin*. *Ribright's* double glasses. The visual glasses of that learned Optician *Mr. Martin*. The square convex form by *Stour*. The patent combined glasses of Messrs. Watkins and Smith, injudiciously called achromatic, consisting of a convex lens, combined with a *meniscus* or concave convex lens. In the various mountings of the frames there is a still greater variety.

The original form most improved.

Notwithstanding these contrivances, universal experience has caused the original and simple form of glasses to supersede them; and it affords an indubitable proof, that it is the best and most convenient that can be devised, when clear glass, accurate tools, and good workmanship are used.

Theorem of Huygens considered as universally applicable.

The theorem given by *Huygens*, and demonstrated by many other subsequent writers on optics, proving that a convex lens having its radii of curvature in the proportion of one to six, has less aberration than any other form of lens, when the greatest convexity is towards the object; and the same for the concave lens; must hold true for any use whatsoever, for which such a formed lens may be required.

Eye glasses of spectacles.

It does not appear to have occurred to *Dr. W.* that the eye glasses used to magnify the images formed by the object glasses in telescopes are of the best form, when with the curves of the proportion above mentioned. In the eye pieces of the best achromatic telescopes, they are always applied, and in high powers, the image frequently subtends an angle from the centre of the eye glass of sixty degrees or more. I have never seen any correct dioptrical theorem, that tended to prove that a *meniscus*, singly, or combined, will answer so perfectly the same purpose.

Remarks on lenses; to ascertain the value of *Dr. Wollaston's* patent spectacles.

The ordinary purposes of vision, are very well answered by the common glasses, under an angle as large as eighty or ninety degrees, and the best artists, or draughtsmen allow, that  $60^{\circ}$  is as much as a fixed position of the eye ought in perspective to embrace, to convey a just representation on the optic nerve.

To persons, the humours of whose eyes are so decayed as to be deficient in their original refractive power, glasses of short foci



foci will, to them, render the extreme parts of objects somewhat confused; but in a much less degree than to persons, with perfect eyes, or undecayed humours.

Remarks on  
lenses; to as-  
certain the value  
of Dr. Wolla-  
ston's patent  
spectacles.

In telescopes and microscopes, the aberration is usually cut off by the insertion of circular apertures or stops; but in spectacles this is not essentially necessary, nor does the want of them or the figure of the glasses, prove that they are constitutionally bad and prejudicial.

The observation of Dr. W. that only a portion of the glass a little larger than the pupil of the eye, is employed at once, is only just inasmuch as it relates to the mind, being intent on a point of an object; but not so in regard to a general view. For the refractive power of the lens does most admirably collect all the infinite number of pencils of rays, or cones, into one concurrence at the pupil of the eye; where they cross or intersect each other: yet, such is the exquisite subtilty of light, that no confusion or irritation takes place. Man is thus blessed by assisted vision, as he is in vitality by the respiration of air.

Dr. W's inferring the form of a meniscus from the shape of a globe, is manifestly erroneous; and in respect to spectacles inappropriate. A glass globe or sphere without any sensible thickness, to an eye exactly placed in its centre, admits all the incident rays to pass throughout it unrefracted. If the eye deviates from the centre, a refraction will take place, and that in proportion to the thickness of the sphere. Rays of incidence pass perfectly unrefracted through truly ground plane, or parallel glass, to an eye before it; and let the axis of the eye, be ever so much inclosed, unless the glass be very thick, the object will still appear perfect, and no refraction of the incident rays will be observed. It is obvious, therefore, the nearer a lens approaches to the figure of a plane, the more perfect it must be.

The figure of a meniscus, which Dr. W. wishes to adopt, is as different from a sphere, as a plane. Its figure is composed of two portions of spheres, of different radii. When with a positive focus it is mathematically demonstrable, that it has entirely the properties of a convex lens, and with a negative focus the properties of a concave one. When the radius of the exterior curve, is less than that of the interior it is a convex sort of lens, and magnifies; but, when the radius of the interior curve is less than that of the other, it is

Remarks on  
lenses; to as-  
certain the value  
of Dr. Wolla-  
ston's patent  
spectacles.

a concave lens, and diminishes. It has also been demonstrated, that the nearer the form of a meniscus approaches to that of a plano convex or concave, the more perfect it will be, and produce less aberration.

I shall dispense here with the proofs by algebraical and analytical formulæ, as any qualified reader will find them in the optical works of *Huygens*, *Molineux*, *Euler*, *D'Alembert*, *Smith*, *Emerson* and *Martin*; and many others.

The rays of light issuing from a near object to a spectacle glass before the eye, are in diverging pencils or cones, and the meniscus form of glass of any certain positive focus, will refract them towards a state of parallelism into the eye, necessary to produce distinct vision, in decayed sight, precisely in the same manner as a double convex or plano convex glass of the same focus would do. A meniscus with a negative focus, acts no ways different from the double or plano concave glasses; the rays of light being divergent somewhat to counteract the effects of too great a convexity in the humours of the eye of a short sighted person. Perhaps, it is hardly necessary to observe, that imperfect vision in the optical sense, consists in the long sighted eye, in the rays of light not being sufficiently converged by its humours, to meet on the retina of the eye, but falling beyond it; and in a short sighted eye by the rays converging too much, so as to meet before they reach the retina.

Varying the geometrical figure of a lens, does not constitute any new optical principle, for any of the common species of lenses, may be cut into the form of a square, a triangle, an oval, &c.-all figuratively various, but consisting only of one optical principle.

The use of the meniscus has been abandoned by Opticians, by its containing in comparison with other lenses, the greatest spherical surface, and consequently producing the greatest aberration. Reducing the curvatures of the meniscus elongates the focus, in the same manner as in other lenses, and therefore reduces the aberration. Hence in spectacle-glasses that are not of short foci, no preceptible difference will be found to persons unacquainted with optical experiments.

There are various practical methods that will point out to persons the aberration of lenses here spoken of, and that the  
meniscus

meniscus causes the greatest of any of the other forms of lenses ; but, the following, I would recommend as the most easy and illustrative.

Remarks on lenses ; to ascertain the value of Dr. Wollaston's patent spectacles.

Take a meniscus lens about the size of a spectacle-glass, and with four inches positive focus, and take also a plano convex or a double convex, of the same diameter and focus, in a room with one lighted candle, at a distance by night, hold the convex glass near to the white wall or wainscot side of the room, between it and the candle; move the lens backwards and forwards till a clear image of the candle is formed, which will be a distinct inverted image of it: do the same with a meniscus, and there will be this difference observed by the meniscus, that incircling the vivid image of the flame, there will be a faint white light, which is the circle of the aberration; and, evidently shews, that it is the worst form of the two for a spectacle-glass, or any other purpose.

Two convex double glasses placed together in one cell, contain less aberration than one glass of the same diameter and focus; and two plano convex-glasses with their convex sides placed together in one cell, give still less aberration. It is loss of light only that can be objected to. They are too weighty to be adopted in spectacles, but in eye pieces of large telescopes for viewing celestial objects, they have been used with great advantage. To engravers, miniature painters, and other artists, they are most useful, as by short foci and large apertures, they give them the most distinct view of a large surface placed before them.

For the satisfaction of an intelligent person, who may be disposed to have an ocular proof of the properties of glasses as herein advanced, I have constructed a frame, containing a double convex, plano convex, a meniscus, two plano convexes, with their convex sides to each other, all of the same diameter, and the foci about four inches, and by which may be seen that the greatest peripheretical indistinctness is with the meniscus glass. This apparatus will be shewn by application at our manufactory in Holborn.

The meniscus as a figure for a spectacle glass I consider very objectionable. To afford a large field of view, its diameter must be considerable; which for a short focus will increase thickness, protuberance outwards, and weight; and, in concave glasses occasion the frames to be made thicker; the glasses will



will be more liable to be scratched and broken than those of the common form, and when the frames are metallic, more likely to increase than diminish that indelible mark made on the nose by the weight of the frame, so frequently complained of by persons who wear spectacles almost constantly. A great deal of superfluous light also passing through the glasses, must be evidently prejudicial; and, it appears to me that the concave figure of the inner side of the meniscus will act as a powerful reflector to condense the rays of light and heat upon the eyes, and ultimately prove thereby of serious injury.

I have in my possession a meniscus spectacle glass, taken from a spectacle frame, which I can prove to have been made many years ago; and, finally, as this form is neither new in principle, or in practice, I am at a loss to know upon what sort of discovery his Majesty's letters patent have been solicited.

I am,

Sir,

Your's, &c.

W. JONES.

### VIII.

*An Account of an Occultation of  $\beta$  Nebulae Sagittarii, by the Planet Mars, on the 17th of April 1796, observed by Sir HENRY ENGLEFIELD, Bart. F. R. S. In a Letter addressed to the Rev. NEVIL MASKELYNE, D. D. F. R. S. and Astronomer Royal.\**

DEAR SIR,

Occultation of a fixed star by the planet Mars.

I BEG leave to communicate to you an observation, which I had the good fortune to make, of an occultation of a fixed star by the planet Mars; and which, as far as I can learn, was seen by nobody in this country but myself and Lady Buck, at whose house at Peterham I made the observation, and to whom I showed the planet when nearly approaching to conjunction with the star.

\*These observations are very rare.

Observations of this kind may be ranked among the rarest phenomena in astronomy. Mr. De La Lande records but

\* From the Journals of the Royal Institution, No. 16, p. 38.

four

four since the invention of telescopes. Of these, one only is by Mars, and that so long ago as about the year 1630, by Gassendi. The curious and very important conjunction of Mars with  $\psi$  aquarii, in October 1672, whence Cassini had hoped to deduce, with great accuracy, the parallax of Mars and the Sun, was missed by bad weather at the moment of the occultation. Since that time, I do not know that any observation of this kind has been made.

The night of April 17, 1796, being very clear, I was led by mere accident to direct my telescope to Mars, about two o'clock in the morning; and I saw with equal pleasure and surprise, a star, about three of his diameters to the east of him, and in a line perpendicular to his horns, he being then gibbous. It was evident that an occultation would take place, and I prepared every thing for the observation.

Account of the observation.

The telescope I used is an achromatic of Ramsden, the aperture 2.75, with a single eye-glass, magnifying 100 times. The clock is a very good regulator, with a wooden pendulum, and to be depended on to less than 2 seconds in the 24 hours. The observations for the time were made with an excellent six inch Hadley's sextant of Ramsden's, and an artificial horizon of mercury.

Instruments.

The position of Petersham, as deduced by a survey made by me some years since, from the Royal Observatory at Kew, which was one of the points settled by General Roy in his great survey, is  $1^{\circ} 12'$  in time west of the Observatory at Greenwich in longitude; and its latitude by the same survey  $51^{\circ} 26' 34''$ .

Place of the obs.

To return to the observation. The sky, during the whole time, was perfectly free from clouds, but was remarkably full of vapours, which at times produced a greater undulation in the limb of Mars, than I almost ever saw. The wind was in the east, and probably the columns of warm air blown from London, mixed very irregularly with the atmosphere of Petersham. This state of the air, and the low altitude of Mars, whose southern declination was  $23^{\circ} \frac{1}{2}$ , rendered it impossible to distinguish with certainty the star, from the protuberances in the waving limb of Mars at the moment of immersion, though the brilliant and silvery light of the star contrasted strongly with the dull red hue of the planet. The observation of the immersion of the star is therefore uncertain, to near a minute

Collateral circumstances.

of

of time. At the emerſion the ſtate of the air was rather more favourable than at the immerſion; and as the ſtar emerged from behind the dark limb of Mars at a ſenſible diſtance from his enlightened edge, this obſervation is much more certain than that of the immerſion; and is probably true, to five, or ten ſeconds at moſt.

Effect of the  
atmoſphere of  
Mars, probably.

The ſtar at its firſt emerſion, certainly appeared much fainter than it did ſoon afterwards; but I cannot pretend to decide, that this apparent diminution of its light was owing to any other cauſe than its great proximity to the ſuperior light of the planet. Yet it was at firſt ſo faint as to be very doubtfully viſible, and in leſs than a minute was very conſiderably brighter. As in this interval of time, its diſtance from the planet was not very much increaſed, it is probable that the atmoſphere of Mars might have been the cauſe, at leaſt in ſome degree, of this appearance.

Oculta-  
tion of a  
fixed ſtar by the  
planet Mars.

*Oculta-  
tion of  $\beta$   $\uparrow$  by the Planet Mars, April 17, 1796,  
at Peterſham.*

Immerſion of  $\beta$   $\uparrow$  per clock . . . . . 15<sup>h</sup> 4' 48"

Uncertain to nearly one minute.

Emerſion of  $\beta$   $\uparrow$  per clock . . . . . 15<sup>h</sup> 24' 28"

Certain to five, or ten ſeconds at moſt.

The ſtar paſſed to the north of the centre of Mars. The immerſion was about one third, or at moſt two fifths of his ſemidiameter north of the centre. The emerſion uot quite half the ſemidiameter north of the centre, by eſtimation.

The clock, at the time of the occultation,

was faſt on mean time . . . . . 0<sup>h</sup> 4' 21 $\frac{1}{2}$ "

The immerſion was therefore at . . . . . 15<sup>h</sup> 0' 26 $\frac{1}{2}$ "

The emerſion at . . . . . 15<sup>h</sup> 20' 6 $\frac{1}{2}$ "

Mean time at Peterſham.

And, Greenwich being 1' 12" in time eaſt of Peterſham, the obſervations reduced to the meridian of Greenwich are,

	Mean Time.	Apparent Time.
Immerſion . . .	15 <sup>h</sup> 1' 31 $\frac{1}{2}$ "	15 <sup>h</sup> 2' 28 $\frac{1}{2}$ "
Emerſion . . .	15 <sup>h</sup> 21' 18 $\frac{1}{2}$ "	15 <sup>h</sup> 22' 8 $\frac{1}{2}$ "

Although this obſervation is ſubject, from the circumſtances ſtated before, to an uncertainty of half a minute in time, as to the middle of the occultation; yet, as the geocentric motion



tion of Mars, was at this time not quite two seconds of a degree in three minutes, his place (that of the star being accurately known); may certainly be determined by this observation within a single second; with a view, therefore, of ascertaining the accuracy of the tables of Mars, given in the last edition of De la Lande's *Astronomy*, the longitude and latitude of Mars were computed with care as follow:

1796, April 17<sup>d</sup> 15<sup>h</sup> 12' 18" apparent time at Greenwich.

	Geoc.	Long.	Geoc.	Lat.	Auft.
Mars	8° 27°	7' 37"	0° 20'	41" 40"	uncorrected,

for aberration or parallax. And April, 18<sup>d</sup> 15<sup>h</sup> 12' 18" his place was 8° 27° 23' 2" 0° 22' 58" 18".

His daily motion in longitude, was therefore 0° 15' 25", and in latitude 0° 2' 16" 38" increasing.

His aberration in longitude at the time of observation, was - 3''.97, and his horizontal parallax, that of the Sun being taken at 8''.72, was 11''.712.

The parallax in longitude and latitude, computed by the tables of the Nonagesimal for Greenwich, given in the *Connaissance des Temps* for 1775 is, Par. Long. + 1''.772, and Par. Lat. + 11''.2.

The apparent place of Mars at the time of the occultation was, therefore, by these tables,

Geoc. Long.	Geoc. Lat. Auft.
8° 27° 7' 34''.8	0° 20' 52''.9.

But, the place of the star, as settled at Greenwich, was at the time of occultation,

Long.	Lat. Auft.
8° 27° 6' 19"	0° 20' 41"

And as the centre of the planet passed about two fifths of his semidiameter south of the star, and the apparent diameter of Mars at that time was very nearly fourteen seconds and an half; the difference of latitude between the star and Mars, may be estimated at three seconds, without possible error of above half a second.

The apparent place then of Mars, as found by observation, was,

Geoc. Long.	Geoc. Lat. Auft.
8° 27° 6' 19"	0° 20' 41"

and the error of the tables is in longitude + 1' 15''.8, and in latitude + 12".

Had

Occultation of a  
fixed star by the  
planet Mars.

Had this observation been made under favourable circumstances, the diameter of Mars might probably have been deduced from it with a great degree of accuracy; although such determination would always have been subject to uncertainty, from the effects of the refraction of his atmosphere, which it is impossible to estimate, but which seems, from some circumstances, to be very considerable.

In the present observation, two other causes of uncertainty exist. The first is, the doubt of the duration of the occultation; the second, the possibility of mistake in the estimation of the distance of the star from the centre of Mars.

The first of these causes cannot induce an error of more than two thirds of a second in the estimate of his diameter. Probably the error is not more than half that quantity.

The probable error arising from the second cause, may amount to about the same quantity as the former. Of this I formed as near a judgment as I could, by drawing Figure 2, *Plate XII.* and placing the path of the star at its utmost limits of distance from the centre by my eye. If the effects of both these causes lie the same way, an error of a second and an half in the determination of the diameter of Mars, may possibly arise; but it is highly probable, that the error is very much less than that quantity, putting the effects of refraction out of the question.

Having, therefore, to the best of my power, compared all the circumstances of the observation, and the effects of the above causes; I consider the equatorial diameter of Mars, as resulting from it, to be at that time  $14''.5$ : and as the distance of Mars from the Earth was then  $0.74456$ , the Sun's mean distance being 1, the diameter of Mars at the mean distance of the Sun, would be  $10''.8$ . Dr. Herschel, in his last paper on Mars, makes it  $9''.8$ .

As all the causes of error in my observation, tend rather to diminish than increase the size of the planet, I am surprised that I should have exceeded his estimate; which is, however, less than that of former astronomers.

This, Sir, is all that has occurred to me on the subject of this very uncommon observation.

You will perceive, that I have not, in my estimations of the diameter of Mars, or of the distances of the star from his limb, made any mention of the spheroidal figure of the planet. The differences in the results would be perfectly insensible,

fible, if that element had been considered; and as I never have been able to perceive the difference of his diameters, I cannot but suspect it to be less than Dr. Herschel supposes.

Occultation of a fixed star by the planet Mars.

I am,

Dear Sir,

Your obliged and faithful,

May 23, 1797.

H. C. ENGLEFIELD.

P. S. In the *Connaissance des Temps* for the year VI. page 457, and year VII. page 428, are accounts of an observation of this occultation; made at Aubenas, or Viviers (for it is not certain at which of these places) by Mr. Flaugergue, member of the National Institute. It is not clear from these accounts that he actually saw the emerfion: at  $15^h 58' 25''$ , apparent time, he saw the star, distant from the limb of the planet only by an interval equal to the supposed breadth of the dark part of the disc of Mars, and it was extremely faint, but perfectly white. Its position was about  $20^\circ$  to the west of the vertical, and above the horizontal diameter. Mr. Flaugergue supposes that the star passed within a second of the northern limb of Mars, and must have been hidden ten minutes and an half.

As the place of observation was in a lower latitude than London, the observer there must have seen the star pass nearer the centre of Mars than I did, the effect of parallax being less, and the occultation must in consequence have lasted longer: but my observation proves, beyond a doubt, that the star passed much nearer the centre than Mr. Flaugergue supposes, as I lost sight of the star at least nineteen minutes and an half.

## IX.

*Letter from Mr. EZEKIEL WALKER, containing a considerable Improvement in Time-Pieces by Mr. BARRAUD, with Remarks.*

To Mr. NICHOLSON,

SIR,

THE method of determining the longitude by time-keepers, possesses some advantages superior to any other that has yet been proposed. In the first place, this method is attended with

The determination of longitude by a time-keeper is more easy and can be oftener



made than by other methods. with very little trouble; and, secondly, the longitude can be found oftener by a time-keeper than by any other means. I have not met with a more striking proof of this position, than in the voyage of La Perouse round the world in the ship La Bouffole. Between the 1st of August 1785, and the 8th of September 1787, this celebrated navigator took only 72 lunar observations; but he took 393 observations on the longitude by his time-keeper No. 19. This is an unequivocal proof of the benefits that navigation derives from this mode of finding the longitude.

Remarkable case in proof. But time-keepers are very expensive, and liable to irregularity. It is to be regretted, however, that time-keepers are expensive and liable to stop, or go irregularly. Were it not for these inconveniences, no other method of finding the longitude need be sought after. But a discovery was made by Mr. Barraud, about the close of the last century, on the effects of oil on time-keepers, which not only reduces them in price, but contributes to their performing with greater precision.

Discovery of Mr. Barraud, by which time-keepers are rendered more perfect, and cheaper. Mr. Barraud had frequently communicated to me his improvements in chronometry, but this discovery on the effects of oil, appeared a matter of so much importance, that I wrote to him, requesting his leave to publish it, and the following extract is taken from his obliging answer.

To Mr. WALKER.

DEAR SIR,

*London, Jan. 28, 1804.*

Letter from Mr. Barraud. I AM much pleased to find that it is your intention to favour the public with your observations on chronometry, and shall derive satisfaction in contributing my mite to so desirable an end: you are therefore welcome to use such information as my experience of facts enables me to furnish.

Jewelling the holes of time-keepers is injurious; The state of my regulator I have already described [in a former letter], from which I infer, that so far from jewelled holes being advantageous in clock-work, they are absolutely injurious. That they are equally so in chronometers, I have had abundant experience, having found, almost without exception, in chronometers coming off a long voyage, the oil in a much worse condition in the jewelled holes (particularly in those where the friction was considerable) than in the brass ones. I have therefore been induced, in every instance, to reject jewelled holes, and introduce those of brass, and the alteration

the oil in such holes being sooner vitiated than in brass holes.

alteration has been constantly favourable to the performance of the time-keeper. I should be happy in having a communication with you *viva voce* on this theme, &c. &c.

I am, &c.

P. P. BARRAUD.

That Mr. Barraud has not made this alteration in his chronometers, in a hasty manner, will appear from the following extract taken from another of his letters.

To Mr. WALKER.

DEAR SIR,

London, July 17, 1800.

SINCE we parted, I have found additional reasons to believe that jewelled holes (where friction is great) are injurious. A box time-keeper which I have recently taken to pieces, on its return from a long voyage, had the oil in the brads holes in a much purer state than in the jewelled ones. In the former it still remained in a state favourable for action, but in the latter, the pivots were so fixed by the tenacity of the oil, as to require force to extricate them; the steel was also deeply stained, and had parted with all its lustre, &c. &c.

Instance of a time-keeper, in which the oil was much more vitiated in the jewelled than in the brads holes:

I am, &c.

P. P. BARRAUD.

From these and many other observations made by Mr. Barraud upon the effects of oil on time-keepers, it appears that small particles of steel are worn off by friction in the jewelled holes, and mixing with the oil, form a glutinous substance that causes the time-keeper to go irregularly.

Most probably by abrasion of the steel pivots.

#### *On the Action of Cold on Oils.*

IT is a known property of some oils, that they freeze much sooner than water. The oil of olives freezes at  $42\frac{1}{2}^{\circ}$  on Fahrenheit's scale, consequently the purest oil of this kind will lose its fluidity sooner than that which contains some aqueous particles. Hence we may conclude, that watch-makers ought to make choice of that oil which freezes with the least degree of cold; and as cold has no power to decompose olive oil, it need not be rejected on account of its having assumed the concrete form.

Olive oil freezes at  $42\frac{1}{2}$  degrees.

In

In consequence of this oil freezing much sooner than water, the following queries seem to claim our attention :

Hence it may perhaps be necessary to adjust and keep time-keepers at temperatures between  $43^{\circ}$  and  $100^{\circ}$ .

*Query 1.* Will a time-keeper go at the same rate when the oil is frozen, that it did when the oil was in a fluid state ?

If this query be answered in the negative, *Query 2.* Would it not then be improper to adjust the compensation balance in frosty weather ?

*Query 3.* Would it not be better to adjust the compensation for the effects of heat and cold at  $43^{\circ}$  for the greatest degree of cold, and at  $96$  or  $100^{\circ}$  for the other extreme ?

A chronometer adjusted in this manner, should never be exposed to a greater degree of cold than  $43$ . This may be easily done with pocket chronometers, but to keep the oil from freezing in box time-keepers, in cold climates, more care may be requisite.

Oil has other properties which ought to be carefully examined before it be applied to time-keepers, but this is an inquiry which must be left until some future opportunity.

I am, Sir,

With much respect,

Your humble servant,

E. WALKER.

## X.

*Letter from C. WILKINSON, Esq. containing Facts upon which Deductions are made to shew the Law of Galvanism in burning the Metals, according to the Disposition of equal Surfaces of charged Metallic Plate.*

To Mr. NICHOLSON,

SIR,

IF the following observations should be deemed worthy of insertion in your valuable Journal, I shall take the liberty of troubling you with some further remarks hereafter.

I am, Sir,

Yours respectfully,

Soho Square.

C. WILKINSON.



When the French philosophers had ascertained that a series of galvanic plates produced effects on animal substances in proportion to the number of plates employed, without any regard to the surface of each plate, they concluded from their experiments, that the effects of a galvanic battery on metallic substances are in proportion to the surfaces of the plates employed. I have lately been engaged in experiments with the most extensive galvanic apparatus hitherto constructed, from which some circumstances have occurred, in some respects militating with the deductions of the French philosophers.

A galvanic trough containing one hundred square plates of four inches in the side, each plate formed of a plate of zinc and copper foldered together; when charged with a solution of nitrous acid and water in the proportion of about 25 parts water to one of acid, exhibited a power capable of igniting half an inch of steel wire of about one seventieth of an inch in diameter.

One hundred galvanic plates of four inches square burned half an inch of wire.

When two such troughs were combined endwise, the power was doubled, and when four were thus arranged, the quantity of wire ignited was quadrupled: hence I ascertained in a very extensive arrangement, that the power is invariably in proportion to the number of plates employed.

Other troughs added endwise burned more wire in proportion to the number of plates.

A galvanic trough consisting of fifty square plates of eight inches each in the side was charged with a similar prepared solution, and this arrangement I found capable of igniting sixteen inches of the same wire as was employed in the former experiment.

A trough of larger plates had much more power,

When two, three, and four troughs of the same size were combined, the lengths of wire ignited proved to be in proportion to the number of plates employed; so that two hundred eight inch plates ignited more than five feet of wire.

—which increased proportionally to the number.

These experiments prove that the powers increase in a greater ratio than as the surfaces of the plates employed. For four hundred plates of four inches, contain an equal surface with one hundred plates of eight inches; and the former will only ignite two inches of the same wire, of which the latter will ignite thirty-two inches.

Examination of the law of increase in the power of larger plates.

If this proportion should be observed in experiments with plates of different size, it will appear that the powers of igniting, as measured by the length of wire, increase, in batteries of the same total surface, as the squares of the surfaces of the elementary plates, singly taken in each.

By equal total surfaces the wire burned is as the square of the elementary surface.

A plate.

Reasoning on the facts. Large plates throw out electricity more rapidly.

A plate of eight inches diameter exposes a surface four times greater than a plate of four inches, and supposing the quantities of electricity given out, to be in proportion to the surface exposed to the chemical action of the fluids, the intensity of (or rapidity of evolution from) each plate of an eight inch trough may be estimated as four times greater than the intensity of electricity from the four inch plates; but the power of ignition is sixteen times greater.

Upon these remarkable circumstances I shall venture to submit to you a few conjectures.

The electricity is thrown out by the parts under the process of oxidation,

A simple galvanic combination resembles in its properties a Leyden phial, that side of the metallic plate which undergoes the greatest change resembles the positive side of the jar, and the other side, the negative, there being no other difference between electricity and galvanism, than the mode in which they are produced. Electricity which appears from the excitation of glass, is occasioned by a temporary change in the capacity of the glass for electricity, while under the rubbing action, being momentarily increased; while galvanism is in some respects the reverse, being occasioned by the diminished capacity of good conducting bodies, when exposed to certain chemical changes. Thus metals which are excellent conductors, when oxydated become non-conductors. While they undergo this change a portion of the combined electricity is evolved, and consequently in the part thus changing positive signs will be evinced.

—and thus by electrophoric action produces a negative state on the unoxidized surface.

The electricity existing in an unchanged metal being perfectly quiescent, is subject, when disturbed, to those laws to which all material substances are amenable; namely of moving in the direction where it meets with the least resistance. As the direction of the electricity is from the substance of the metal towards the surface undergoing the chemical change, it must follow that the other side of the metallic plate which is defended from the action of the fluid will evince negative signs, while the side acted upon positive, nearly upon the same principles as those of the electrified plates.

The discharge is by a succession of correspondent pairs of plates,

In effecting a discharge of the galvanic battery, the circuit must be completed between the zinc and copper ends of the battery. The two plates nearest the points of contact are first concluded; thus the two next, and so on in successive parts, each plate at equal distances from the points of contact; and the

the last pair which is unloaded, are the two plates in the centre of the trough. In an arrangement of only 50 or 60 plates, the rapidity of each successive discharge appears to be so great as not to admit of the intervals being distinguished by our sense of feeling; but when the number is very considerable the sensation produced is jarring and tremulous, such as we may conceive would arise from the action of a constant current of a subtile fluid.

—consequently the whole discharge of equal surfaces is more rapidly made the fewer the plates,

When we expose metallic substances to the galvanic action, the effects produced are greater from a large surface diffused over a small number of plates, than when spread over a greater number of plates; if we suppose 800 particles of electricity given out from a plate of eight inches in diameter, there would only be 200 particles from a plate of four inches (supposing the quantity in the ratio of the surfaces exposed) if two plates be unloaded at once, we shall have from one arrangement 1600 particles of electricity determined in one mass, and from the other only 400; from the former we have a rapid communication of a quantity of electricity of four times the intensity of the latter. And as metallic substances nearly transmit the whole, the effects produced are found to be as the squares of the intensities.

—and the quantity of electricity actually passing in the wire at any instant of time is greater.

I am now constructing a battery of fifty plates, each plate of which will be two feet in diameter. If the above circle should hold good, the extent of wire that will be ignited will be very considerable. A battery of 50 plates, each plate eight inches in diameter, ignites 16 inches of steel wire of one seven-tieth of an inch in diameter. And as the plates I am now preparing will expose a surface nine times greater, I shall expect that  $16 \times 9 \times 9 = 1296$  inches, or 108 feet of wire will be ignited at every contact.

A battery now in hand expected to explode above 100 feet of wire.



## XI.

*Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double-stars; with an Investigation of the Cause to which they are owing. By WILLIAM HERSCHEL, LL. D. F. R. S. From the Philosophical Transactions for 1803.*

*Read June 9, 1803.*

General observations on the theory of the heavenly bodies.

IN the Remarks on the Construction of the Heavens, contained in my last Paper on this subject,\* I have divided the various objects which astronomy has hitherto brought to our view, into twelve classes. The first comprehends insulated stars.

As the solar system presents us with all the particulars that may be known, respecting the arrangement of the various subordinate celestial bodies that are under the influence of stars which I have called insulated, such as planets and satellites, asteroids and comets, I shall here say but little on that subject. It will, however, not be amiss to remark, that the late addition of two new celestial bodies, has undoubtedly enlarged our knowledge of the construction of the system of insulated stars. Whatever may be the nature of these two new bodies, we know that they move in regular elliptical orbits round the sun. It is not in the least material whether we call them asteroids, as I have proposed; or planetoids, as an eminent astronomer, in a letter to me, suggested; or whether we admit them at once into the class of our old seven large planets. In the latter case, however, we must recollect, that if we would speak with precision, they should be called very small, and exzodical; for, the great inclination of the orbit of one of them to the ecliptic, amounting to 35 degrees, is certainly remarkable. That of the other is also considerable; its latitude, the last time I saw it, being more than 15 degrees north. These circumstances, added to their smallness, show that there exists a greater variety of arrangement and size among the bodies which our sun holds in subordination, than we had formerly been acquainted with, and extend our knowledge of the construction of the solar, or insulated sidereal system. It will not be required that I should add any

\* See Phil. Transf. for 1802, p. 477, and our Journal, V. 72, thing

thing farther on the subject of this first article of my classification; I may therefore immediately go to the second, which treats of binary sidereal systems, or real double stars.

General observations on the theory of the heavenly bodies.

We have already shewn the possibility that two stars, whatever be their relative magnitudes, may revolve, either in circles or ellipses, round their common centre of gravity; and that, among the multitude of the stars of the heavens, there should be many sufficiently near each other to occasion this mutual revolution, must also appear highly probable. But neither of these considerations can be admitted in proof of the actual existence of such binary combinations. I shall therefore now proceed to give an account of a series of observations on double stars, comprehending a period of about 25 years, which, if I am not mistaken, will go to prove, that many of them are not merely double in appearance, but must be allowed to be real binary combinations of two stars, intimately held together by the bond of mutual attraction.

It will be necessary to enter into a certain theory, by which these observations ought to be examined, that we may find to what cause we should attribute such changes in the position, or distance, of double stars, as will be reported; and, in order to make the required principles very clear, I shall give them in a few short and numbered sentences, that they may be referred to hereafter.

In *Plate. X. Fig 1*: let us call the place of the sun, which may also be taken for that of the observer, O. In the centre of an orbit or plane N F S P is  $\alpha$  Geminorum; and, if any other star is to be examined, we have only to exchange the letter  $\alpha$  for that by which such double star is known. This letter is always understood to represent the largest of the two stars which make up the double star; and a general expression for its smaller companion will be  $x$ . N, F, S, P, represent the positions of the different parts of the heavens, with respect to  $\alpha$ , north, following, south, and preceding; and the small letters  $n, f, s, p$ , stand for the same directions with respect to O.  $x \alpha P$ , is the angle of position of the two stars  $x$  and  $\alpha$ , with the parallel F P.

Theory of the motions of the stars.

As the motion of an observer affects the relative situation of objects, we have three bodies to consider, in our investigation of the cause of the changes which will be pointed out; the sun, the large star, and the small star, or, as we have shortly called

Theory of the  
motions of the  
stars.

them, O,  $\alpha$ ,  $x$ . This admits of three cases : a motion of one of the three bodies ; another, of two ; and a third, of all the three bodies together. We shall now point out the consequences that will arise in each of the cases.

*Single Motions.*

No. 1. Motion of  $x$ . When  $\alpha$  and O are at rest, the motion of  $x$  may be assumed, so as perfectly to explain any change of the distance of the two stars, and of their angle of position.

No. 2. Motion of  $\alpha$ . When  $x$  and O are at rest, and  $\alpha$  has a motion, either towards P, N, F, or S, then the effect of it, whatever may be the angle P  $\alpha$  O, will be had by entering the following Table, with the direction of the given motion.

Motion.	Distance.	Angle.	Quadrants.
$\alpha$ P	$\begin{array}{c} - \\ + \end{array}$	$\begin{array}{c} + \\ - \end{array}$	1st and 4th 2 — 3
$\alpha$ F	$\begin{array}{c} + \\ - \end{array}$	$\begin{array}{c} - \\ + \end{array}$	1 — 4 2 — 3
$\alpha$ N	$\begin{array}{c} - \\ + \end{array}$	$\begin{array}{c} - \\ + \end{array}$	1 — 2 3 — 4
$\alpha$ S	$\begin{array}{c} + \\ - \end{array}$	$\begin{array}{c} + \\ - \end{array}$	1 — 2 3 — 4

No. 3. Motion of O. 1st case. When  $\alpha$  and  $x$  are at rest, and the angle P  $\alpha$  O is 90 degrees, a proper motion of O, towards either  $p, f, n$ , or  $s$ , which will be extremely small when compared with the distance of O from  $\alpha$ , can have no effect on the apparent distance, or angle of position, of the two stars ; and therefore no other motion, composed of the directions we have mentioned, will induce a change in the comparative situation of  $\alpha$  and  $x$ .

2d case. When the plane P N F S is oblique to the ray  $\alpha$  O, and the angle P  $\alpha$  O more than 90 degrees, the effect of the motion of O will be had by the following Table.

Motion



Theory of the  
motions of the  
stars.

Motion.	Distance.	Angle.	Quadrants.
$Op$	+	—	1st and 2d 3 — 4
$Of$	—	+	1 — 2 3 — 4
$On$	$\begin{smallmatrix} + \\ - \end{smallmatrix}$	$\begin{smallmatrix} + \\ - \end{smallmatrix}$	$\begin{smallmatrix} 1 — 3 \\ 2 — 4 \end{smallmatrix}$
$Os$	$\begin{smallmatrix} - \\ + \end{smallmatrix}$	$\begin{smallmatrix} - \\ + \end{smallmatrix}$	$\begin{smallmatrix} 1 — 3 \\ 2 — 4 \end{smallmatrix}$

3d case. When the angle  $P \propto O$  is less than 90 degrees, the following Table must be used.

Motion.	Distance.	Angle.	Quadrants.
$Op$	—	+	1st and 2d 3 — 4
$Of$	+	—	1 — 2 3 — 4
$On$	$\begin{smallmatrix} - \\ + \end{smallmatrix}$	$\begin{smallmatrix} - \\ + \end{smallmatrix}$	$\begin{smallmatrix} 1 — 3 \\ 2 — 4 \end{smallmatrix}$
$Os$	$\begin{smallmatrix} + \\ - \end{smallmatrix}$	$\begin{smallmatrix} + \\ - \end{smallmatrix}$	$\begin{smallmatrix} 1 — 3 \\ 2 — 4 \end{smallmatrix}$

#### Double Motions.

No. 4. If we admit different motions in two of our three bodies, and if the ratio of the velocities, the directions of the motions, and the ratio of the distances of the bodies be given quantities, a supposition in which we admit their concurrence, may explain the phenomena of a double star, but can never be probable

#### Motions of the three Bodies.

No. 5. If we admit different motions in every one of the three bodies,  $O \propto x$ , and if the velocities and directions of the motions,

Theory of the  
motions of the  
stars.

motions, as well as the relative distances of the three bodies are determined, an hypothesis which admits the existence of such motions and situations, may resolve the phenomena of a double star, but cannot have any pretension to probability.

The compass of this Paper will not allow me to give the observations of my double stars at full length; I shall therefore, in the examination of every one of them, only state those particulars which will be required for the purpose of investigating the cause of the changes that have taken place, either in the distance, or angle of position, of the two stars of which the double star is composed.

As the arguments in the case of most of these stars will be nearly the same, it may be expected, that the first two or three which are to be examined will take up a considerable space, and the number of double stars, in which I have already ascertained a change, amounting to more than fifty, it will not be possible to give them all in one paper; I shall therefore confine the present one to a moderate length, and leave it open for a continuation at a future opportunity.

#### *α Geminorum.*

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From my earliest observations on the distance of the two stars which make up the double star in the head of Castor, given in the first of my catalogues of double stars, we find, that about 23 years and a half ago, they were nearly two diameters of the large star asunder. These observations have been regularly continued, from the year 1778 to the present time, and no alteration in the distance has been perceived: the stars are now still nearly two diameters of the large one asunder.

It will be necessary to enter a little into the practicability of ascertaining distances by a method of estimation apparently so little capable of precision. From a number of observations and experiments I have made on the subject, it is certain that the apparent diameter of a star, in a reflecting telescope, depends chiefly upon the four following circumstances: the aperture of the mirror with respect to its focal length; the distinctness of the mirror; the magnifying power; and the state of the atmosphere at the time of observation. By a contraction of the aperture we can increase the apparent diameter of a star, so as to make it resemble a small planetary disk. If distinctness should be wanting, it is evident that the image of objects will not be sharp  
and

and well defined, and that they will consequently appear larger than they ought. The effect of magnifying power is, to occasion a relative increase of the vacancy between two stars that are very near each other; but the ratio of the increase of the distance is not proportional to that of the power, and sooner or later comes to a maximum. The state of the atmosphere is perhaps the most material of the four conditions, as we have it not in our power to alter it. The effects of moisture, damp air, and haziness, (which have been related in a paper where the causes that often prevent the proper action of mirrors were discussed,) show the reason why the apparent distance of a double star should be affected by a change in the atmosphere. The alteration in the diameter of Arcturus, extending from the first to the last of the ten images of that star, in the plate accompanying the above-mentioned paper,\* shows a sufficient cause for an increase of the distance of two stars, by a contraction of their apparent disks. A skilful observer, however, will soon know what state of the air is most proper for estimations of this kind. I have occasionally seen the two stars of Castor, from  $1\frac{1}{2}$  to 2 and  $2\frac{1}{2}$  diameters asunder; but, in a regular settled temperature and clear air, their distance was always the same. The other three causes which affect these estimations, are at our own disposal; an instance of this will be seen in the following trial. I took ten different mirrors of seven feet focal length, each having an aperture of 6,3 inches, and being charged with an eye-glass which gave the telescope a magnifying power of 460. With these mirrors, one after another, the same evening I viewed the two stars of our double star; and the result was, that with every one of them, the stars were precisely at an equal distance from each other. These mirrors were all sufficiently good to show minute double stars well; and such a trial will consequently furnish us with a proper criterion, by which we may ascertain the goodness of our telescope, and the clearness of the atmosphere required for these observations. To those who have not been long in the habit of observing double stars, it will be necessary to mention, that, when first seen, they will appear nearer together than after a certain time; nor is it so soon as might be expected, that we see them at their greatest

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\* See Phil. Trans. for 1803, page 232, Plate III.

distance.



Observations and distance. I have known it to take up two or three months, inferences re- before the eye was sufficiently acquainted with the object, to specting the judge with the requisite precision. changes of re-  
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other.

Whatever may be the difficulties or uncertainties, attending the method of determining the distance of two close stars by an estimation of the apparent diameter, it must however be confessed, that we have no other way of obtaining the same end with so much precision. Our present instance of  $\alpha$  Geminorum, will show the degree of accuracy of which such estimations are capable, and at the same time prove, that the purpose for which I shall use the estimated interval between the two stars will be sufficiently answered. By an observation of the 10th of May, 1781, we have the diameter of the largest of the two stars to that of the smallest as 6 to 5; and according to several measures I have taken with the micrometer, we may admit their distance, diameters included, to be five seconds. Then, as the vacancy between the two stars is nearly, but not quite two diameters of the large one, I shall value it at  $1\frac{1}{2}$ . From this we calculate, that the diameter of the large star, under the circumstances of our estimation, is nearly  $1''.35$ : so that an error of one quarter of such a diameter, which is the most we can admit, will not exceed  $0''.34$ . Nor is it of much consequence, if the measure of  $5''$  should not be extremely correct; as a small mistake in that quantity will not materially affect the error of estimation by the diameter, which, from what has been said, if the measure was faulty to a second, would not amount to more than one-fifteenth part of it.

Having thus ascertained that no perceptible change in the distance of the stars has taken place, we are now to examine the angle of position. In the year 1779, it was  $32^{\circ} 47'$  north preceding; and, by a mean of the three last measures I have taken, it is now only  $10^{\circ} 53'$ . In the space of about 23 years and a half, therefore, the angle of position has manifestly undergone a diminution, of no less than  $21^{\circ} 54'$ ; and, that this change has been brought on by a regular and gradual decrease of the angle, will be seen when the rest of the measures come to be examined.

The accuracy of the micrometer which has been used, when the angles of position were taken, being of the utmost importance, it becomes necessary to ascertain how far it will be safe to rely on the result of the measures. It might be easily shown

known that, in the day time, a given angle, delineated on a card, and stuck up at a convenient distance, may be full as accurately measured by a telescope furnished with this micrometer, as it can be done by any known method, when the card is laid on a table before us; but this would not answer my purpose. For, objects in motion, like the stars, especially when at a distance from the pole, cannot be measured with such steadiness as those which are near us, and at rest. The method of illuminating the wires, and other circumstances, will likewise affect the accuracy of the angles that are measured, especially when the distance of the stars is very small. I shall therefore have recourse to astronomical observations, in order to see what the micrometer has actually done.

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January 22, 1802. The position of A Orionis was taken. 1st measure,  $52^{\circ} 38'$  south preceding; 2d measure,  $54^{\circ} 14'$ . Mean of the two measures,  $53^{\circ} 26'$ . Deviation of the measures from the mean,  $48'$ .

March 4, 1802. 11 Monocerotis. 1st measure,  $28^{\circ} 18'$  south following; 2d measure,  $26^{\circ} 49'$ . Mean of the two,  $27^{\circ} 34'$ . Deviation from the mean,  $45'$ .

February 9, 1803.  $\alpha$  Geminorum. 1st measure,  $6^{\circ} 11'$  north preceding; 2d measure,  $4^{\circ} 48'$ . Mean of the two,  $5^{\circ} 29'$ . Deviation from the mean,  $41'$ .

September 6, 1802.  $\gamma$  Coronæ. 1st measure,  $89^{\circ} 42'$  north following; 2d measure,  $89^{\circ} 38'$ . Mean of the two,  $89^{\circ} 40'$ . Deviation from the mean,  $2'$ .

When these observations are considered, we shall not err much if we admit that, in favourable circumstances, and with proper care, the micrometer, by a mean of two measures, will give the position of a double star true to nearly one degree; but, as the opportunities of taking very accurate measures are scarce, it will be necessary to have recourse to some more discordant observations.

February 18, 1803.  $\beta$  Orionis. 1st measure,  $72^{\circ} 58'$  south preceding; 2d measure,  $67^{\circ} 24'$ . Mean of the two,  $70^{\circ} 11'$ . Deviation from the mean,  $2^{\circ} 47'$ .

But a memorandum to the observation says, that the evening was not favourable. We may therefore admit, that in the worst circumstances which can be judged proper for measuring at all, an error in the angle of position by two measures will not amount to three degrees.

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It will be remarked, when we come to compare single measures which have been taken on different nights, that they are somewhat more discordant; but I have not ventured to reject them on that account, except in cases where it was pretty evident that some mistake in reading off, or other accident to which all astronomical observations are liable, was to be apprehended. Nor can such disagreements materially affect the conclusions I have drawn, when it appears that the deviations happen sometimes to be on one side, and sometimes on the other side, of the true angle of position. For, since that angle is not a thing that will change in the course of a few nights, the excess of one measure will serve to correct the defect of another; and we are not to think it extraordinary, when stars are so near together, and their motion through the field of view (in consequence of the high magnifying power we are obliged to use) so quick, that we should now and then even fall short of that general accuracy which may be had by a careful use of the micrometer.

I shall now enter into an examination of the cause of the change in the angle of position of the small star near Castor.

A revolving star, it is evident, would explain in a most satisfactory manner, a continual change in the angle of position, without an alteration of the distance. But this, being a circumstance of which we have no precedent, ought not to be admitted without the fullest evidence. It will therefore be right to examine, whether the related phenomena cannot be satisfactorily explained by the proper motions of the stars, or of the sun.

#### *Single Motions.*

(a) The three bodies we have to consider, are O,  $\alpha$ , and  $x$ ; and, supposing them to be placed as they were observed to be in the year 1779; the angle  $x \alpha P$ , in Fig. 1, will be  $32^\circ 47'$  north preceding. We are at liberty to let the angle  $P \alpha O$  be what will best answer the purpose. Then, in order to examine the various hypotheses that may be formed, according to the arrangement of the principles we have given, we shall begin with No. 1; and, as this admits that all phenomena may be resolved by a proper motion of  $x$ , let us suppose this star to be placed any where far beyond  $\alpha$ , but so as to have been seen, in the year 1779, where the angle of position,  $32^\circ 47'$  north preceding



ceding, and the observed distance, near 2 diameters of the large star, required it. With a proper velocity, let it be in motion towards the place where it may now be seen at the same distance from Castor, but under an angle of position only  $10^{\circ} 53'$  north preceding. It may then be admitted, that a small decrease of the distance which would happen at the time when the angle of position was  $21^{\circ} 50'$ , could not have been perceived; so that the gradual change in the observed angle of position, as well as the equality of the distance of the two stars, will be sufficiently accounted for. But the admission of this hypothesis requires, that  $\alpha$  Geminorum and the solar system should be at rest; and, by the observations of astronomers, which I shall soon have occasion to mention, neither of these conditions can be conceded.

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(b) If, according to No. 2, we admit the motion of  $\alpha$ , we shall certainly be more consistent with the observations which astronomers have made on the proper motion of this star\*; and, as a motion of the solar system, which I shall have occasion to mention hereafter, has not been rigidly proved, it may, for the sake of argument, be set aside; nor has a proper motion of the star  $x$  been any where ascertained. The retrograde annual proper motion of Castor, in right ascension, according to Dr. Maskelyne, is  $0''.105$ . This, in about  $23\frac{1}{2}$  years, during which time I have taken notice of the angle of position and distance of the small star, will amount to a change of nearly  $2''.47$ . Then, if we enter the short Table I have given in No. 2, with the motion  $\alpha P$ , we find, that in the first quadrant, where the small star is placed, the distance between the two stars will be diminished, and the angle of position increased. But since it appears, by my observations, that the distance of the stars is not less now than it was in 1780; and that, instead of an increase in the angle of position, it has actually undergone a diminution of nearly 22 degrees; it follows, that the motion of  $\alpha$  Geminorum in right ascension, will not explain the observed alterations in the situation of this double star. If,

\* See Tobizæ Mayeri *Opera inedita. De motu fixarum proprio*, page 80. Also Dr. Maskelyne's first Volume of Observations. Explanation and Use of the Tables, p. iv. Or Mr. Wollaston's Astronomical Catalogue, end of the Preface. Likewise *Connoissance des Temps pour l'Année VI.* p. 203. *Sur le Mouvement particulier propre à différentes Etoiles;* par Mons. De la Lande.

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according to Mr. De la Lande's account\*, we should also consider the annual proper motion of  $\alpha$  in declination, which is given  $0''.12$  towards the north, we shall find, by entering our Table with the motion  $\alpha$  N, amounting to  $2''.82$ , that the distance of the two stars will be still more diminished; but that, on the contrary, the angle of position will be much lessened; and, by combining the two motions together, the apparent disks of the two stars should now be a little more than one-tenth of a second from each other, and the angle of position 35 degrees south preceding. But, since neither of these effects have taken place, the hypothesis cannot be admitted.

(c) That the sun has a proper motion in space, I have shown with a very high degree of evidence, in a paper which was read at the Royal Society about twenty years ago†. The same opinion was before, but only from theoretical principles, hinted at by Mr. De la Lande, and also by the late Dr. Wilson, of Glasgow‡; and has, since the publication of my paper, been taken up by several astronomers§, who agree that such motion exists. In consequence of this, let us now, according to No. 3. assign to the sun a motion in space, of a certain velocity and direction. Admitting therefore  $\alpha$  and  $x$  to be at rest, let the angle  $P\alpha O$  be 90 degrees; then, by the 1st case of No. 3, we find that none of the observed changes of the angles of position will admit of an explanation. There is moreover an evident concession of the point in question, in the very supposition of the above angle of 90 degrees; for, if  $x$  be at the same distance as  $\alpha$  from the sun, and no more than  $5''$  from that star, its real distance, compared to that of the sun from the star, will be known; and, since that must be less than the 40 thousandth part of our distance from Castor, these two stars must necessarily be within the reach of each others attraction, and form a binary system.

(d) Let us now take the advantage held out by the 2d case of No. 3, which allows us to place  $x$  far behind  $\alpha$ ; in which

\* See page 211 of the treatise before referred to.

† *Philos. Trans.* vol. lxxiii. p. 247.

‡ See my note in *Phil. Trans.* vol. lxxiii. p. 283.

§ See *Astronomisches Jahrbuch für das Jahr 1786; Seite 259. Über die Fortrückung unseres Sonnen-Systems, von Herrn Professor Prevost. Und für das Jahr 1805; Seite 113.*

situation, the angle  $P\alpha O$  will be more than 90 degrees. The star  $x$  being less than  $\alpha$ , renders this hypothesis the more plausible. Now, as a motion of Castor, be it real or apparent, has actually been ascertained, we cannot set it aside; the real motion of  $O$ , therefore, in order to account for the apparent one of  $\alpha$ , must be of equal velocity, and in a contrary direction: that is, when decomposed,  $0'',105$  towards  $f$ , and  $0',12$ , towards  $s$ . The effect of the sun's moving from  $O$  towards  $f$ , according to the 1st Table in No. 3, is, that the distance between the two stars will be diminished, and the angle of position increased. But these are both contrary to the observations I have given. The motion of  $O$  in declination towards  $s$ , according to the same Table, will still diminish the distance of the two stars, but will also diminish the angle of position. Then, since a motion in right ascension increases the angle, while that in declination diminishes it, the small star may be placed at such a distance that the difference in the parallax, arising from the solar motion, shall bring the angle of position, in  $23\frac{1}{2}$  years, from  $32^\circ 47'$  to  $10^\circ 53'$ ; which will explain the observed change of that angle. The distance of the star  $x$ , for this purpose, must be above  $2\frac{1}{2}$  times as much as that of  $\alpha$  from us. But, after having in this manner accounted for the alteration of the angle of position, we are, in the next place, to examine the effect which such a difference of parallax must produce in the apparent distance of the two stars from each other. By a graphical method, which is quite sufficient for our purpose, it appears, that the union of the two motions in right ascension and declination, must have brought the two stars so near, as to be only about half a diameter of the large star from each other; or, to express the same in measures, the centers of the stars must now be  $1'',8$  nearer than they were  $23\frac{1}{2}$  years ago. But this my observations cannot allow; for we have already shown, that any change of more than 3 or 4-tenths of a second must have been perceived.

If, on the other hand, we place the star  $x$  at such a distance that the solar parallax may only bring it about 4-tenths of a second nearer to  $\alpha$ , which is a quantity we may suppose to have escaped our notice in estimating the apparent distance of the two stars, then will the angle of position be above 20 degrees too large. This shows, that no distance, beyond Castor, at which we can place the star, will explain the given observations.

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(c) The last remaining trial we have to examine, is to suppose  $x$  to be nearer than  $\alpha$ ; the angle  $P\alpha O$ , will then be less than 90 degrees; and the effect of a motion of  $O$  towards  $f$ , by the 2d Table in No. 3, will be an increase of the distance of the two stars, and a diminution of their angle of position. But the motion  $Ox$ , which is also to be considered, will add to the increase of the distance and counteract the diminution of the angle. It is therefore to be examined, whether such an increase of distance as we can allow to have escaped observation, will explain the change which we know to have happened in the angle, during the last  $23\frac{1}{2}$  year. By the same method of compounding the two motions as before, it immediately appears, that we cannot place the small star more than about 1-tenth of the distance  $O\alpha$  on the side of Castor, without occasioning such an increase of the apparent distance of the two stars as cannot possibly be admitted; and that, even then, the angle of position, instead of being less, will be a few degrees larger, at the end of  $23\frac{1}{2}$  years, than it was at the beginning. This hypothesis, therefore, like all the foregoing ones, must also be given up, as inconsistent with my observations.

It is moreover evident, that the observations of astronomers on the proper motion of the stars in general, will not permit us to assume the solar motion at pleasure, merely for the sake of accounting for the changes which have happened in the appearances of a double star. The proper motion of Castor, therefore, cannot be intirely ascribed to a contrary motion of the sun. For we can assign no reason why the proper motion of this star alone, in preference, for instance, to that of Arcturus, of Sirius, and of many others, should be supposed to arise from a motion of the solar system. Now, if they are all equally intitled to partake of this motion, we can only admit it in such a direction, and of such a velocity, as will satisfy mean direction and velocity of the general proper motions of the stars; and place all deviations to the account of a real proper motion in each star separately.

#### *Double Motion.*

(f) In order to explain the phenomena of our double star, according to No. 4, by the motion of two bodies, for instance  $\alpha$  and  $x$ , it will be required that they both should move in given directions;

directions; that the velocities of their motions should be in a given ratio to each other; and that this ratio should be compounded with the ratio of their distances from O; a supposition which must certainly be highly improbable. To shew this with sufficient evidence, let us admit that, according to the best authorities, the annual proper motion of Castor is  $0''\cdot105$  in right ascension, and  $0''\cdot12$  in declination towards the north. Then, as the small star, without changing its distance, has moved through an angle of  $21^\circ 54'$ , the only difference in the two motions of these stars, will be expressed by the extent of the chord of that angle. To produce the required effect, it is therefore necessary that the motion of  $\alpha$ , which is given, should regulate that of the small star, whose relative place at the end of  $23\frac{1}{2}$  years is also given. Then, as  $\alpha$  moves in angle of  $53^\circ 31'$  north preceding, and with a velocity which, being expressed by the space it would describe in  $23\frac{1}{2}$  years, will be  $3''\cdot51$ , it is required that  $x$  shall move in an angle of  $29^\circ 25'$ , likewise north preceding, and with a velocity of  $3''\cdot02$ . The ratio of the velocities, therefore, and the directions of the motions, are equally given. But this will not be sufficient for the purpose: their distance from O must also be taken into consideration. It has been shown, that the two stars cannot be at an equal distance from us, without an evident connection; it will therefore be necessary for those who will not allow this connection, to place one of them nearer to us than the other. But, as the motions which have been assumed, when seen from different distances, will subtend lines whose apparent magnitudes will be in the inverse ratio of the assumed distances, it is evident that this ratio, if the motions are given, must also be a given one; or that, if the distances be assumed, the ratio of the motions must be compounded with the ratio of the distances. How then can it be expected that such precise conditions should be made good, by a concurrence of circumstances owing to mere chance? Indeed, if we were inclined to pass by the difficulties we have considered, there is still a point left which cannot be set aside. The motion of the solar system, although its precise direction and velocity may still be unknown, can hardly admit of a doubt; we have therefore a third motion to add to the former two, which consequently will bring the case under the statement contained in our 7th number, and will be considered hereafter.

(g) If

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(g) If we should intend to change our ground, and place the two motions in O and  $x$ , it will then be conceded, that the motion of  $\alpha$  is only an apparent one, which owes its existence to the real motion of the sun. By this, the effect of the solar parallax on any star at the same distance will be given; and it cannot be difficult to assume a motion in  $x$ , which shall, with the effect of this given parallax, produce the apparent motion, in the direction of a chord from the first to the last angle of position pointed out by my observations; taking care, however, not to place the stars  $\alpha$  and  $x$  at the same distance from us; using the inverse ratio of the solar parallax as a multiple in the assigned motion. For instance, let the sun have a motion of the velocity expressed as before by  $3'',51$ , and in a direction which makes an angle of  $53^\circ 13'$  south following with the parallel of  $\alpha$  Geminorum; and let the small star  $x$  have a real motion in an angle of  $18^\circ 40'$  south preceding from the parallel of its situation, and with a real velocity which, were it at the distance of  $\alpha$ , would carry it through  $8'',89$ . Then, if the distance of the small star be to that of the large one as 3 to 2, the effect of the solar parallax upon it will be  $\frac{2}{3}$  of its effect upon  $\alpha$ ; that is, while  $\alpha$ , which is at rest, appears to move over a space of  $3'',51$ , in an angle of  $53^\circ 31'$  north preceding, the parallactic change of place in  $x$  will be  $2'',34$  in the same direction. This, though only an apparent motion, will be compounded with the real motion we have assigned to it, but which, at the distance of  $\alpha$ , will only appear as  $1'',26$ ; and the joint effect of both will bring the star from the place in which it was seen  $23\frac{1}{2}$  years ago, to that where now we find it situated.  $\alpha$ , in the same time, will appear to have had an annual proper motion of —  $0'',105$  in right ascension, and  $0'',12$  in declination towards the north; and thus all phenomena will be explained.

From this statement, we may draw a consequence of considerable importance. If we succeed, in this manner, in accounting for the changes observed in the relative situation of the two stars of a double star, we shall fail in proving them to form a binary system; but, in lieu of it, we shall gain two other points, of equal value to astronomers. For, as  $\alpha$  Geminorum, according to the foregoing hypothesis, is a star that has no real motion, its apparent motion will give us the velocity and direction of the motion of the solar system; and, this

being



being obtained, we shall also have the relative parallax of every star, not having a proper motion, which is affected by the solar motion. Astronomical observations on the proper motion of many different stars, however, will not allow us to account for the motion of  $\alpha$  Geminorum in the manner which the foregoing instance requires; the hypothesis, therefore, of its being at rest, must be rejected.

(h) If we place our two motions in O and  $\alpha$ , we shall be led to the same conclusion as in the last hypothesis. The known proper motion of  $\alpha$ , and the situations of the small star in 1779 and 1803, given by my observations, will ascertain the apparent motion of  $x$ , now supposed to be at rest. Then, since the change in the place of  $x$  must be intirely owing to the effect of parallax, it will consequently give us, in the same manner as before, the quantity and direction of the motion of the solar system, and the relative distances of all such stars as are affected by it. But, here again, the solar motion required for the purpose is such as cannot be admitted; and the hypothesis is not maintainable.

#### *Motion of the three Bodies.*

(i) There is now but one case more to consider, which is, according to No. 5, to assign real motions to all our three bodies; and this may be done as follows. Suppose the sun to move towards  $\lambda$  Herculis, with the annual velocity 1.

Let the apparent motion of  $\alpha$  Geminorum be as it is stated in the astronomical tables before mentioned; but suppose it to arise from a composition of its real motion with the effect of the systematical parallax, as we may call that apparent change of place of stars which is owing to the motion of the solar system. Let the real motion of  $x$ , aided by the effect of the same parallax, be the cause of the changes in the angle of position which my observations have given. We may admit the largest of the two stars of our double star to be of the second magnitude; and, as we are not to place  $x$  too near  $\alpha$ , we may suppose its distance from O to be to that of  $\alpha$  from the same as 3 to 2. In this case, O will move from the parallel of  $\alpha$ , in an angle of  $60^\circ 37'$  north following, with an apparent annual velocity of ,4536. The motion of  $\alpha$  in right ascension, may be intirely ascribed to solar parallax; but its change of declination, cannot be accounted for in the same manner. Let us therefore

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admit that the solar velocity, in the direction we have calculated, will produce an apparent retrograde motion in  $\alpha$ , which, in  $23\frac{1}{2}$  years will amount to  $2''.085$  in right ascension. But the same parallax will also occasion a change in declination, towards the south preceding, of  $3''.701$ ; and, as this will not agree with the observed motion of  $\alpha$ , we must account for it by a proper motion of this star directly towards the north. The real annual velocity required for this purpose, must be  $1,3925$ .

The apparent motion of  $x$ , by parallax, at the distance we have placed this star, will be  $2''.832$  towards the south preceding; and, by assigning to it an annual proper motion of the velocity  $1,3354$ , in the direction of  $73^\circ 10'$  north preceding its own parallel, the effect of the solar parallax and this proper motion together, will have caused the small star, in appearance, to revolve round  $\alpha$ , so as to have produced all the changes in the angle of position which my observations have given; and, at the same time,  $\alpha$  will have been seen to move from its former place, at the annual rate of  $0''.105$  in right ascension, and  $0''.12$  in declination towards the north.

In this manner, we may certainly account for the phenomena of the changes which have taken place with the two stars of  $\alpha$  Geminorum. But the complicated requisites of the motions which have been exposed to our view, must surely compel every one who considers them to acknowledge, that such a combination of circumstances involves the highest degree of improbability in the accomplishment of its conditions. On the other hand, when a most simple and satisfactory explanation of the same phenomena may be had by the effects of mutual attraction, which will support the moving bodies in a permanent system of revolution round a common centre of gravity, while at the same time they follow the direction of a proper motion which this centre may have in space, it will hardly be possible to entertain a doubt to which hypothesis we ought to give the preference.

As I have now allowed, and even shown, the possibility that the phenomena of the double star Castor may be explained by proper motions, it will appear that, notwithstanding my foregoing arguments in favour of binary systems, it was necessary, on a former occasion, to express myself in a conditional manner,\*



ner,\* when, after having announced the contents of this Paper, I added, “*should these observations be found sufficiently conclusive;*” for, if there should be astronomers who would rather explain the phenomena of a small star appearing to revolve round Castor by the hypothesis we have last examined, they may certainly claim the right of assenting to what appears to them most probable.

(To be continued.)

## XII.

*On the Nature of the Varieties of Engrafted Fruit-Trees, with a Plan for increasing the Number of New valuable Fruits.* By T. S. DYOT BUCKNALL, Esq. M. P†.

TO MR. CHARLES TAYLOR.

SIR,

SOME friends have requested that I would introduce another Introduction.  
 Paper on the Nature of the valuable Varieties of engrafted Fruits, as they are of opinion that the Essay in the 17th Volume of the Transactions of the Society is not sufficiently extended for a subject so important to the Fruit-growers, and those interested in the productions of Fruits. As a proof of my willingness to make the Orchardist as perfect as I can, I beg you to present my compliments to the Society, with the following elucidations.

This is a subject in rural oeconomy which ought to be much Importance.  
 better understood than it is, in order to enable the planters to judge of the sorts proper to be planted, either as an article of pleasure, profit, or recreation; as much of the credit of the plantation must arise from judiciously choosing Trees of the best, new, or middled-aged sorts, and not of the old worn-out varieties, which latter cannot, in the planting of Orchards in common situations, ever form *valuable Trees*, and must end in the disappointment of the planter.

Engrafted Fruits, I have before said, and I now repeat, are Grafted fruits are not permanent.  
 not permanent. Every one of the least reflection must see that

\* See Phil. Trans. for 1802, page 486.

† Society of Arts, xx. 144.



there is an essential difference between the power and energy of a seedling plant, and the tree which is to be raised from cuttings or elongations. The seedling is endued with the energies of nature, while the graft, or scion, is nothing more than a regular elongation, carried perhaps through the several repetitions of the same variety; whereas the seed, from having been placed in the earth, germinates and becomes a new plant, wherever nature permits like to produce like in vegetation; as in the oak, beech, and other mast-bearing trees. These latter trees, from each passing through the state of seedlings, are perfectly continued, and endued with the functions of forming perfect seeds for raising other plants by evolution, to the continuance of the like species.

The varieties continued by grafting last only for a time.

This is not the case with engrafted fruits. They are doomed by nature to continue for a time, and then gradually decline, till at last the variety is totally lost, and soon forgotten, unless recorded by tradition, or in old publications.

Reason, with which Providence has most bountifully blessed some of our species, has enabled us, when we find a superior variety, to engraft it on a wilding stock, or to raise plants from layers and cuttings, or even to raise up the roots, and thus to multiply our sources of comfort and pleasure. This, however, does not imply that the multiplication of the same variety, for it is no more, should last for ever, unless the species will naturally arise from seed.

Process of raising fruits, and increasing them by grafting.

Nature, in her teaching, speaks in very intelligible language, which language is conveyed by experience and observation. Thus we see that among promiscuous seeds of fruits of the same sort, one or more may arise, whose fruits shall be found to possess a value far superior to the rest in many distinguishable properties. From experience, also, we have obtained the power, by engrafting, of increasing the number of this newly-acquired tree, can change its country, give it to a friend, send it beyond the seas, or fill a kingdom with that fruit, if the natives are disposed so to do. Thus we seem to have a kind of creative power in our own hands.

From the attention lately paid to the culture of engrafted Fruits, I hope we are now enabled to continue a supposed happily acquired tree, when we can find it, for a much longer duration than if such variety had been left in the state of unassisted nature; perhaps I may say for a duration as long again,

or

or something more. After these sanguine expectations, I may reasonably be asked, to what does all this amount? for here there is no direct permanency—and why? The *why* is very obvious—because the kernels within the fruit, which are the seed of the plants for forming the next generation of trees, will not produce their like. I allow they may do so accidentally; but nothing more can be depended on.

Process of raising  
fruits and in-  
creasing them by  
grafting.

For example, suppose we take ten kernels or pips of any apple raised on an engrafted stock: sow them, and they will produce ten different varieties, no two of which will be alike; nor will either of them closely resemble the fruit from whence the seeds were collected. The leaves also of those trees raised from the same primogenious or parent stock, will not *actually* be a copy of the leaves of any one of the varieties or family, to which each is connected by a vegetable consanguinity. I intentionally used the word *actually*, because a resemblance may be found, though not much of that is to be expected.

I beg that what has been last mentioned may not be taken as a discouragement to attempts for raising new varieties. I was obliged to speak very strongly, in order to place the culture upon its true foundation. I think it need not be observed, that there is no acquiring a new variety, but through the means of a seedling plant; and therefore whoever wishes to succeed must attempt it that way, or wait till others in their plantations may more fortunately produce it.

In choosing the seeds, that apple is most likely to produce the clearest and finest plants, whose kernels are firm, large, and well ripened. The size of the fruit is not to be regarded; for large apples do not always ripen their fruit well, or rather for cider the small fruits are generally preferred for making the strongest, highest-flavoured liquor. And from what I have been able to collect in the cider-countries, it is there the opinion, that an apple something above the improved crab promises the best success. This advantage also attends the practice: if there are no valuable apples raised from that attempt, these wildings will make excellent stocks to engraft upon.

Gentlemen who actually employ themselves in attempting to acquire new varieties, should remember that they ought to select all the sets, from the bed of apple-quick, whose appearance is in the least degree promising, and plant them together, at such a distance as to allow each to produce its fruit, which

will

will happen in about twelve, fifteen, or eighteen years. My friend Mr. Knight, who undoubtedly is the first in actual exertions for procuring these happily acquired new varieties, has had two plants bear fruit at six years old, and one at five. The cider-countries have offered several premiums for procuring new varieties, and some with good effect. Premiums have been given both to Mr. Knight and Mr. Alban.

When the new variety is to be raised from a valuable *admired* apple, I should recommend the placing these seeds in a garden-pot, filled with mould from an old melon-bed; carrying the pot into a retired situation near the water, and giving attention to run the plants to as large a size as is convenient within eighteen months. With this view, the pot should be placed in the green-house the first winter; and when the plants are afterwards to be set out in the spots, they should not be placed under the drip of trees, or much exposed to the winds.

Two instances have been mentioned, the improved crab, and most admired apple; but prudence says, try all sorts, and something probably will arise; and the process is attended with little trouble or expence to a person who constantly resides in the country: yet, after all this scientific care, the apple may want flavour, and be in other respects nothing better than a common wilding.

It is an undoubted fact, and worthy of observation, that all the different trees of the same variety have a wonderful tendency to similarity of appearance among themselves; and that the parent stock, and all engrafted from it, have a far greater resemblance to each other, than can be found in any part of the animal creation; and this habit does not vary to any extent of age.

Facts respecting  
engrafted fruits,  
their degree of  
permanence, &c.

As an encouragement in attempting to increase the number of new valuable fruits, we can prove that the golden pippin is native English. The red-streak, a seedling of Herefordshire, if not raised, yet was first brought into notice by Lord Scudamore, and was for a long time called Scudamore's Crab. The Stire Apple was accidentally raised in the Forest of Dean, in Gloucestershire, and took the name of *Forest Stire*. The cider made from this apple was the strongest the country ever produced, according to any living record. The Haglo-crab, the best cider fruit now remaining, was discovered



in the parish of Ecloe, on the banks of the Severn; and, about sixty or seventy years ago, many scions were taken from this tree by Mr. Bellamy, and engrafted on seedling stocks about Rofs. These are now grown old; and, to ascertain the age of the variety, I went with Charles Edwin, Esq. to Ecloes, in hopes of seeing the primogenious of this family. The proprietor of the estate acquainted Mr. Edwin that it had ceased to bear years ago, and was cut down. Those at Rofs are but poor bearers now, and I should suppose the variety must be 140 years old, though Marshall, who wrote in the year 1786, mentions these trees were prolific, and he supposes the sort to be about eighty years old; but, from present experience, it must be much more. The Tinton Squash-pear is of Gloucestershire; the Barland and Old-field were near Ledbury, Herefordshire. The two last pears clearly bear the names of the two fields where they were raised. The Barland fell about six years ago, visibly from weight and longevity, which was supposed to have been about 200 years. There had been many other names of estimation handed down to us, though the realities are now totally worn out, and have ceased to exist. Can any better proof be desired, that engrafted fruits are not permanent, then the regret we feel for the loss of these old valuable fruits.

To make my paper as short as convenient, I have dwelt only on the apple and pear; yet all the engrafted fruits are under the same predicament of the seed not producing its like, and the offspring in time falling into a nothingness of growth and bearing, though that space of time must certainly depend on the natural longevity and hardness of the sort, soil, position, care, &c. All these are more fully expressed in the papers published in the different volumes of the Transactions of this Society, and the two volumes of the Orchardist, wherein the whole system is extended, to form a rational culture for the management of Standard Fruits.

It should be remembered, that as I am now alluding to the state of actual permanency, fifty years are to be accounted as nothing; and as often as we come to that point, we are compelled to resort to our first assertion, "That engrafted fruits are not permanent, they being continued from elongations, and not raised as a repetition of seeds." This is the only rational

Facts respecting  
engrafted fruits,  
their degree of  
permanence, &c.

Facts respecting engrafted fruits, their degree of permanence; &c. rational way as yet introduced of accounting for the loss of the valuable old varieties of fruits. Should a better system be introduced, I shall readily adopt it; but this sufficiently answers the purposes of the planter.

Some years ago, from due investigation and thorough conviction, I propagated this principle; and it was published in the 17th volume of the Society's Transactions, in the following words: "All the grafts taken from this first tree, or parent stock, or any of the descendants, will for some generations thrive; but when this first stock shall, by mere dint of old-age, fall into actual decay, a nihility of vegetation—the descendants, however young, or in whatever situation they may be, will gradually decline; and, from that time, it would be imprudent, in point of profit, to attempt propagating that variety from any of them. This is the dogma which must be received. I do not expect a direct assent, neither do I wish it, for it should be taken with much reserve; but it is undoubtedly true." These considerations should stimulate us in searching after new varieties, equal, or perhaps superior to those of which we regret the loss.

Observe that, from the time the kernel germinates for apple-quick, should the plant be disposed to form a valuable variety, there will appear a regular progressive change, or improvement, in the organization of the leaves, until that variety has stood, and grown sufficient to blossom and come into full bearing; that is, from the state of infancy to maturity; and it is this and other circumstances, by which the inquisitive eye is enabled to form the selection among those appearing likely to become valuable fruits. But from that time the new variety, or selected plant, compared with all the engraftments which may be taken from it, or any of them, these shall shew a most undeviating sameness among themselves.

It is readily allowed, that the different varieties of fruits are easily distinguished from each other by many particulars, not only respecting their general fertility, and the form, size, shape, and flavour of the fruit, but also the manner of the growth of the tree, the thickness and proportion of the twigs, their shooting from their parent stem, the form, colour, and consistence of the leaf, and many other circumstances, by which the variety can be identified; and were it possible to engraft

engraft each variety upon the same stock, they would still retain their discriminating qualities, with the most undeviating certainty.

Facts respecting engrafted fruits, their degree of permanence; &c.

The proper conclusion to be drawn from the statement in the last paragraph, is this—that were any one to put the thought in practice on a full-grown hardy or crab-stock, it would produce an excellent proof that engrafted fruits are not permanent. For if twenty different varieties were placed together, so that each might receive its nurture from the same stem, they would gradually die off in actual succession, according to the age or state of health of the respective variety, at the time the scions were placed in the stock; and a discriminating eye, used to this business, would nearly be able to foretell the order in which each scion would actually decline. Should it also happen that two or three suckers from the wilding stock had been permitted to grow among the *twenty grafts*, such suckers or wilding shoots will continue, and make a tree after all the rest are gone. A further consequence would result from the experiment: among such a number of varieties, each of the free growers would starve the delicate, and drive them out of existence only so much the sooner. It must be observed, that this supposed stem is the foster-parent to the twenty scions, and real parent to the suckers; and those the least conversant with engrafted fruits know the advantage acquired from this circumstance. And here it is worth while remarking, that a Gascoyne, or wild cherry, will grow to twice the size that ever an engrafted cherry did.

By an experiment we have had in hand for five years, it will appear that the roots and stem of a large tree, after the first set of scions are exhausted or worn out, may carry another set for many years; and we suspect a third set, provided the engrafting is properly done, and the engrafter chooses a new variety. Now the Ripston pippin, of Yorkshire, is the favourite, as being a free grower and good bearer, with fine fruit. This however may be certainly depended on, that when a new apple is raised from seed, if a scion were placed in a retired situation, and constantly cut down, as a stool in a copse-wood, and the apple never suffered to fulfil the intentions of nature in bearing fruit, the practitioners of the following ages may secure scions from that stool, to continue the variety much longer. Hence, though I have written as much as is in  
my



Facts respecting my power against permanency, yet I have taken some pains engrafted fruits, to assure the planters, that forecast, selection, pruning, clean-  
 their degree of permanence, &c. lines, and care, will make the orchards turn to more profit, for the rising generations, than what they have done for the last hundred years.

To place the nature of varieties in its true light, for the information of the public, I must maintain, that the different varieties of the apple will, after a certain time, decline, and actually die away, and each variety, or all of the same stem or family, will lose their existence in vegetation; and yet it is a known fact, and mentioned in the 17th volume of the Transactions, that after the debility of age has actually taken possession of any variety, it will yet thrive by being placed against a southern wall, and treated as wall-fruit. Who, however, can afford to raise cider at that expense, except as matter of curiosity, to prove, that when the vital principle in vegetation is nearly exhausted, a superior care and warmth will still keep the variety in existence some time longer?

It should be understood, that the external air of Britain is rather too cold for the delicate fruits, which is the reason why, in the Orchardist, I lay such a stress on procuring warmth for the trees, by *draining, shelter, and manure*. It would be now lost time to attempt to recover the old varieties as an article of profit.

If I have not expressed myself, in this Essay on the Nature of Varieties, with so much clearness and conviction as might have been expected, it should be considered that it is an abstruse subject, very little understood, and requiring at first some degree of *faith, observation, and perseverance*. The prejudices of mankind revolt against it. They are not disposed to allow the distinction of nature; and they imagine, that in the act of engrafting or multiplying they give new life, whereas it is only continuing the existence of the same tree, stick, or bud. Observe what I said before:—the seed of the apple, when placed in the earth, germinates, and unfolds itself into a new plant, which successively passes through the stages of infancy, maturity, and decay, like its predecessors. I might say, all created nature is similar in this respect; though, from the circumstance that varieties are much longer-lived than man, the plants have appeared to be possessed of  
 eternal

eternal powers of duration: nothing sublunary, however, which possesses either animal or vegetable life, is exempt from age and death.

Facts respecting  
engrafted fruits,  
their degree of  
permanence,  
&c.

Within the last twenty years I have travelled many hundred miles, and conversed with the most intelligent men in each country; and I now want to convince mankind, for no other reason than because it is their interest so to believe, that there is in creation an order of beings (engrafted fruits) so formed, that we have the power of multiplying a single variety, to whatever number of trees we please;—that the first set arises from a small seed;—that the next and descendent sets are propagated by engraftings, or from cuttings, layers, &c.;—and that although these trees may amount to millions, yet, on the death of the primogenious or parent stock, merely from old-age, or nihility of growth, each individual shall decline, in whatever country they may be, or however endued with youth and health. I say they shall gradually begin to decline; and in the course of time, or of centuries, to those who would prefer that expression, the *whole variety* will scarcely have a single tree remaining to show what the fruit was. Let those who are not disposed to assent to this statement, ask themselves what is become of the old lost varieties? did they die, or did wicked men maliciously cut them up?

I, who am firmly convinced of the truth of what I have advanced on this subject, have no doubt but that the same would happen by engrafting on the Oak or Beech, if the mast raised from the engrafted tree did not produce the like; for there the question turns.

Is it not known, that the woodman, in setting out his sapling oaks, always selects new seedling plants, and never continues one upon an old stool; and that if he should so blunder, that tree, from the stool, will neither have the freedom of growth, nor the size or firmness of timber, equal to a new-raised plant.

I wish I could persuade my friends, that, with the same attention with which the woodman acts, the planter is to raise his orchard from the young fruits which thrive in the neighbourhood, or are in health and full bearing in the country whence they are to be brought.

The fruit-grower should look to selection, cleanliness, and care. To me it is a circumstance perfectly indifferent, whether

Facts respecting their he is to use Mr. Forsyth's composition, Mr. Bulingham's engrafted fruits, boiled linseed oil, or my medication. I only maintain that the their degree of wounded parts of trees want something to destroy the insects permanence, and vermin, and heal the wood, from which the trees are kept &c. in health.

Let those who are blessed with fruit-plantations attend to their preservation, and not leave them to the state of unassisted nature.

I am, Sir,

Your most obedient Servant,

THO. SKIP DYOT BUCKNALL.

Hampton-Court,  
12th Oct. 1801.

### XIII.

*Explanation of the Subjects concerning which Questions were proposed at Page 71 of the present Volume; namely, 1. The Place of the Erect Image behind a Concave Speculum; 2. Of the Image formed by a Concavo Convex Mirror, which is not Left-handed, and has the Property of revolving along with the Mirror; and 3. The Figure of the Sky. By W. N.*

Answers to certain questions.

AS the questions of my correspondent R. B. have not been treated by any other of my friends, I shall here discuss them according to my promise.

Erect image in a concave mirror.

1. The question respecting the erect image in the concave mirror relates to a phenomenon which formed the subject of discussion in print very early in the last century, though I cannot at this moment recollect the authors who have related the facts and arguments. R. B. is perfectly correct as to the various divergence of the rays composing the pencils which issue from radiant points at different distances from the eye, but it does not appear that our notions of distance are greatly re-

Our judgment of distance is not founded on the nature of the pencils of light;

gulated by that circumstance. Our judgment of the distances of near objects is partly founded on the adjustment required to be made in the eye itself to produce distinct vision by the rays of the several pencils, as R. B. remarks, but much more on a circumstance commonly noticed by authors, but by him overlooked, namely, the degree of convergence between the optical axes of the two eyes which is required to avoid squinting,

or



or the phenomenon of two images. In proof of this, if he will take two pens or pencils or other pointed objects, and, shutting one eye, endeavour to apply their points end-wise to each other by moving them across the line of sight, he will find how much less precisely one eye can give a judgment on distances, than both eyes. And our judgment of distant objects is established not at all upon the nature of the pencils, but on the perspective arrangement or angular magnitude of the objects together with their gradual obscuration (from the interposed mass of air) called aerial perspective. Hence we find that the several parts of a well drawn sketch of mere lines, and still more of an excellent painting, give all the notions of distance, though there cannot be any difference in the divergence of the pencils of light to correspond with that effect. The consciousness that a body is approaching or retiring arises little if at all from any change in the pencils; in near objects that notion is rather gained from the required change in the optical axes, and in these and all distant objects it is almost entirely produced by the angular magnitude becoming larger or smaller, while we contemplate the appearances.

These truths will explain the image in the concave mirror: For 1. When the face is near the mirror, the image is seen by a great convergence of the optical axes, which becomes less and less as the observer retires, at the same time that the image itself subtends a less and less angle:—It therefore, for both reasons, appears to retire. 2. At a greater distance the image seems stationary while the observer retires, because the angular enlargement of the image is nearly compensated by its diminution from increased distance; and 3. At a still greater distance the angular enlargement increases so fast that the image seems rapidly to come forward until it is lost in confusion.—If both eyes be kept open, the observer has a considerably accurate notion of the real distance of the frame, which, together with the confusion of two different images presented to the eyes, may lead him to have the sentiment of a mere angular enlargement; but if one eye be covered, the notions of retreat, stationary position, and rapid advance of the image will infallibly be adopted.

2. With regard to the concavo-convex mirror, none of the pencils of light are brought truly to a focus. If we conceive the surface of the mirror to be divided into zones by lines

but on the situation of the eye, and the perspective.

The image in a concave mirror seems to retire and advance from these causes.

Explanation of  
the effects of a  
concavo-convex  
mirror.

drawn perpendicularly across the shortest arc of convexity that passes through the vertex; and these lines to be indefinitely near each other;—or to speak less mathematically, if the mirror were divided into small parallel stripes, drawn right across its concavity;—each of those small zones or stripes will reflect the light of any distant object to a focus before the mirror without any sensible error; whence it will diverge in a kind of flat pencil; and these pencils themselves will not be parallel to each other, but will diverge from a point behind the mirror or virtual focus; so that this last divergence of the pencils will be at right angles to the divergence of the rays that compose them. If therefore we attend to the portion of light which enters the eye from what may be taken to be a radiant point of the image, in this case, we shall perceive that they cannot be brought to a correct focus on the retina. For if the eye be adjusted to the place of convergence before the mirror, the focal spot will be elongated in the direction of the arc of convexity; and on the contrary, if the eye be adjusted to the virtual focus of the convex, or point of divergence, the rays of the pencils will not be duly collected, but will render the spot on the retina oblong in the direction of the arc of concavity. Nevertheless, if the distance of the eye be so great, as that the difference of adjustment for these two points shall not produce any sensible effect of this kind, the whole image will appear sufficiently neat and distinct, though a little deformed; that is to say, those dimensions which are governed by the concavity, will be somewhat greater than those governed by the convexity; because as we may affirm, the concave mirror is nearer the eye than the convex. From this general explanation, it will be easily understood that the image must disappear and become utterly confused by the quantity of cross aberration, when any attempt is made to examine it with a convex lens; and that it will be impossible to form an image of the sun or of any other object upon the surface of paper or other similar material. In fact the reflected light from the sun considered as a luminous point, will converge before the mirror to a line or caustic curve of which I have not yet considered the properties; but which, if the concave mirror were cylindrical, would be a right line at the distance of half the radius from the vertex.

It is not necessary after what has been said to enter into any fuller explanation of the reason why the image is inverted, (that is to say either right handed or upside-down) with regard



to the relative position of its parts considered across the concavity, and the contrary along the lines of convexity;—or, while these lines have their position altered by rotation of the mirror, the image itself will also appear to revolve with twice the velocity. These effects, as well as that of the extreme distortion of the image, seen when the object is within the centre of concavity, will be easily apprehended by the reader, whose notions of optics are sufficient to enable him to understand the preceding paragraph.

3. The enlarged appearance of the sun and moon at low altitudes or near the horizon, and the apparent flatness of the concavity of the sky have been explained, as my correspondent observes, by reference to the diminished light of the heavenly bodies, when it passes through a long portion of the atmosphere, and likewise to the notions of distance which are obtained by looking over a long row of terrestrial objects. That is to say the explanation is grounded on the supposition or notion of greater distance along the horizontal line deduced from consideration of the aerial and the geometrical perspective. The observations and objections of R. B. appear to me to be perfectly well founded. I think the effect is produced almost entirely by the geometrical perspective, not of the houses, trees, and other land objects, but of the clouds themselves. When the clouds are in low distinct flakes with clear openings between them, the angular magnitude both of the clouds and their intervals will be greater near the zenith, and will diminish as the zenith distance encreases, so as on some occasions to exhibit the appearance of objects running out to an immense distance in the concavity of an extremely flat dome. On the contrary, when lofty towering masses of clouds rise from the horizon to a considerable elevation facing the setting sun, or otherwise so circumstanced that the tint of the whole mass shall be very little varied, the mind, so far from adopting the notion of an extended dome in that part, shall receive the impression of an immensely elevated wall, with very little curvature as it rises. Both these conditions are not unfrequently to be seen at one and the same time, and the sky is then, according to the observation of R. B. very far from exhibiting a figure of regular dimensions. A great variety of intermediate forms and groups of the clouds, often present themselves, by which the apparent figure of the sky will be made to differ from these extreme cases. When the sky is perfectly and closely clouded, we

Why the sky appears flat, and the sun and moon large in the horizon.

have



have perhaps very little notion of an apparent figure unless from our former habits, or from the angular motion produced by the wind. This will be greatest near the zenith, and by shewing us that the clouds over our heads are nearer than the others, will give us a conviction of the figure of a flat arch.

*Useful Notices respecting various Objects. By a Correspondent.*

*Cotton in Ink.*

R. B.

Cotton in ink  
recommended.

THE ancient practice of putting cotton in ink is almost entirely given up. But when we consider that the colouring matter of ink is merely a precipitate, in the act of slow subsidence, and that the gum as well as the Gallic acid, are subject to speedy decomposition or mouldiness by exposure to the air, we shall see good reasons for resuming it. The black fecula is kept suspended by the cotton; the fluid is prevented from circulating by heat, cold or mechanical agitation, and consequently presents a very small surface to the air; and lastly the method of dipping tends to keep the pen clean and in good condition. I can take upon me to assert that ink in cotton is blacker, more durable, and much less liable to become mouldy. The latter effect may be almost entirely prevented by occasionally turning the cotton upside down, that is to say, every two or three days.

*Shaving with Water.*

Shaving with  
water.

IN some of your early numbers the operation of shaving has been rather amply discussed by yourself and correspondents. I have nothing to add in the way of reasoning to what has been there brought forward; but I take this opportunity of noticing a fact. Long ago it was observed to me that the soap answers no other purpose than that of clearing the skin, in proof of which it was asserted that the face so cleaned may be shaved quite as well without the lather as with it. I made trial of this process, but did not find it succeed, because I wiped the skin dry. But I have since found that if the beard be well washed with soap, and then with clear water, the operation of shaving may be very effectually and pleasantly performed, while the water continues upon the skin. The effects are that the razor cuts more keenly and closely, and the skin seems to be much less tender. I leave your other correspondents to reason and discover the cause of the excellence of this method; in the mean time I shall continue to use it, unless a still more considerable improvement should be made.

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# A JOURNAL

## OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

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APRIL, 1804.

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### ARTICLE I.

*Experiment proving the Advantage of Periscopic Spectacles. Communicated in a Letter from W. H. WOLLASTON, M. D. F. R. S.*

To Mr. NICHOLSON,

SIR,

THE opinion given by Mr. Jones, in your last Journal, respecting the improved form of spectacle-glasses, on which I had delivered my sentiments in the number preceding (p. 144,) induces me to address you once more upon that subject.

It appears wholly unnecessary to follow Mr. J. through his various observations, or to remark upon the experiment by which he deceives himself; because all doubt of the advantage of the periscopic glasses may be removed by the following direct comparative trial, which any person who chooses can make without difficulty.

I have before me two glasses, each of four inches "positive focus," as proposed by Mr. J. the one double-convex, which in his judgment is pronounced to be "indubitably the best and most convenient that can be devised" (p. 194); the other a concavo-convex, or meniscus, which he thinks "evidently the worst of the two for a spectacle-glass," (p. 197.)

When I fix the former at the distance for most distinct vision opposite to a printed octavo page, and approach my eye to the

Mr. Jones's opinion of the improved spectacle glasses,

referred to experiment.

With the form of lens recommended by Mr. Jones,

the extent of distinct vision compared with that by the

periscopic glass  
was as 24 to 40,

glass, I cannot without pain read quite twenty-four lines; but upon substituting the periscopic glass fixed in the same position, I can with *ease* discern every word in the page, which contains forty lines.

—or rather as  
1 to 3.

The enlargement of the field of view observable in this trial is sufficient to evince the superior utility of the periscopic glass; but were there occasion to compare more nearly the circular areas that may be seen with equal distinctness, they would be found to differ by a ratio as great as that of three to one.

A corresponding  
advantage in  
shallower lenses.

This difference is of course more evident in glasses of so high power than in those generally used by long-sighted persons for common purposes; but it cannot be doubted, that a corresponding, though smaller, inequality subsists wherever there is the same dissimilarity of construction, even when the focal distance is longest.

Conclusion.

The advantage in question is therefore indisputably proved by direct experiment; to the *novelty* of it Mr. J. himself has unintentionally contributed very satisfactory evidence; but as to its importance, those alone, who have the misfortune to labour under any defect of vision, must ultimately decide.

I remain, Sir,

Your obliged humble servant,

March 20, 1804.

W. H. WOLLASTON.

## II.

*Additional Observations on some remarkable Strata of Flint in the Isle of Wight, in a Letter from Sir CHARLES ENGLEFIELD, Bart. F. R. S. to JOHN LATHAM, M. D. F. R. S. and L. S.\* (See Page 183 of the present Number.)*

DEAR SIR,

Observations on  
remarkable  
strata of flint in  
the Isle of  
Wight.

I FEEL much flattered at the notice taken of my Paper on the chalk pits in the Isle of Wight by the Linnean Society; and as I wish to render my account of the very curious appearances observed by me in them as perfect as I can, the following additional observations on the subject made during a second visit last year are, by your favour, submitted to the Society.

\* Linnean Transactions, Vol. VI.



The pits I last year inspected are as follow, beginning from the east:

Brading pit, which is at the eastern point of the great ridge, where the valley of Brading-haven intersects it, and separates it from the Yaverland-hill, which terminates in the sea at Culver and Bembridge. A road cut into the chalk above Knighton. Ashley-down pit, about three miles east of Newport. A pit very near to and south of Carisbrook castle. The cliffs and caves of Freshwater bay, both east and west of the valley which intersects them entirely and runs from Freshwater to Yarmouth.

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strata of flint in  
the Isle of  
Wight.

The Yaverland chalk is therefore the only part which I have not examined; and little doubt can be entertained of its similarity to the rest of the range, to which it evidently belongs.

In Brading pits some flints appear in detached nodules, and these are found and unbroken.

The inclined strata of flint are visible, but not to advantage, owing to the manner of working the pits. In these strata the flints are universally shattered, some into absolute powder, others into grosser powder and fragments mixed. But besides these strata, the chalk in this pit is divided by vast perpendicular fissures, as smooth as plaster walls, and in some of these fissures flint has formed, which appears broken like that in the strata.

The road above Knighton only just cuts into the chalk stratum, but all the flints visible in the banks are extremely shattered.

The pit at the west end of Ashley-down, near two large barrows, is the most extensive and satisfactory of any I have seen. The perpendicular face of the chalk, where worked, is not less than fifty or sixty feet, and its direction is at right angles to that of the strata, and parallel to their line of dip:—of course they are seen to very great advantage. The strata seems to dip northward more rapidly than in any other place where I could observe them. The angle of inclination is from 75 to 80 degrees. There are not layers of flints between every layer of the chalk. Some of the chalk is peculiarly solid, and rises in very large masses affecting a cubic form. Their solid vein is from twenty-five to thirty feet thick, and is in strata from three to four feet. In all this solid part there are very few flints.

Both above and below this harder bed (speaking of the original position of the strata) the chalk is softer, and has more

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flints in it. The stratified flints in this pit are full as much shattered as any I had seen. The nodules are not at all broken. Many of the stratified flints are much defaced in this pit by an admixture of pyrites, so as to be quite opaque like a coarse jasper, and these flints are much softer than the others, as is always the case in the impure flints.

In the chalk pits near Carisbrook the strata are not so visible as in the pits north of the castle (described in the first papers) but the flints are to the full as finely, though perhaps not so generally broken. In one flint I observed that though it lay in its bed undisturbed, chalk as if in a fluid state had run into one of the fissures. Every appearance of this pit indicates that the chalk, since its stratification, has received a most violent shock.

The chalk at Freshwater bay appears in high perpendicular cliffs, particularly on the western side of the bay. Both on the east and west the strata dip northward near 80 degrees, and the dip seems to run east and west very regularly. The western cliff has a very regular and perpendicular face to the eastward; and here the parallel direction of the strata, each separated by a thin line of black flint presents a most curious appearance. The flint here is often found in thin plates of considerable extent, sometimes not above an inch thick, and seems formed from each side of the space which it fills, as the exterior parts (or those nearest the chalk) are the purest and blackest, and it is gradually whiter towards the middle, where there is often a soft line of chalk included between the two plates of flint. All the stratified flints are more or less shattered, and some are reduced to very fine powder. The cave at Freshwater, which is really a beautiful as well as a very curious one, is formed by the action of the sea on these nearly vertical strata. They are of different hardness, and all intersected with fissures at right angles to the strata. When the sea acts on and wears away a soft stratum, a gallery is formed, and the upper parts of the stratum between fissure and fissure drop out, much in the same way as bricks are apt to do out of the flat arch over a window, the harder contiguous strata serve as walls to the gallery, but are by degrees perforated in different parts, and become irregular pillars, supporting the vast weight of the hills above, until the action of the sea weakens them so far that they fall, and a part of the face of the hill goes with them, so that the cave is constantly, although slowly, changing its form.

Larger masses of the harder strata, defended by their flint coating, also stand up in the bay as insulated rocks of different shapes, and much resembling the Needle Rocks, which are exactly of the materials and formed by the same process. In this part of the chalk stratum, I saw several fossils remain which I had sought in vain in the pits I had visited. One was singular, it had the appearance of part of a very large shell, regularly striated, and almost flat. I have often seen small fragments apparently of a similar shell in chalk, but never a large piece. It was so firmly fixed as not to be removed without a chissel, which I had not.

Observations on remarkable strata of flint in the Isle of Wight.

To these observations on the chalk of the island, I must add, that this whole range, although really chalk, is much harder than the chalk of the South-downs, insomuch that the carpenters cannot use it for drawing lines, but import chalk for that purpose from Portsdown hill above Portsmouth. They also call the island chalk by the name of marle, which is, however only the British name for chalk, and appears in many compound names, such as Marl-borough on the Wiltshire chalk hills, and the very significant one of Albemarle, or white chalk.

As I have made some further observations on the southern range of hills which form the back of the island, I will trespass on your patience a little longer, particularly as they in some degree contradict, or rather correct, what I had advanced on that subject in my former letter.

When the northern front of those hills is viewed from Ashley down, the stratum of stone mentioned in my former letter as lying directly under the chalk of St. Catharine's and Dunnose hills, appears every where to maintain an horizontal position; and so in its general position, particularly in its northern front, it certainly does, but just behind the village of Ventnor, the stratum entirely disappears, as if it had been ingulphed in a great chasm, and a deep and narrow valley runs winding into the chalk hill of St. Boniface, though it does not penetrate through it, which seems the remains of the fissure into which the stone had sunk.

The appearances of the great stone stratum, from Niton eastward to Ventnor are noted as follows in the journal made on the spot.

On an attentive inspection of the strata of the under cliff, it appears that the great stratum of rugged and laminated stone, which



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remarkable  
strata of flint  
in the Isle of  
Wight.

which first appears at the west side of St. Catharine's, and thence ranging eastward, forms the front of the cliff, overhanging the Underway dips in its southern face gently to the eastward. The cliffs at Mirables are much higher above the sea than those at St. Laurence, and from thence they decline, till at the opening of the hill above Ventnor they totally disappear. A small crag just peeps out of the eastern face of this dell, and the whole hill of St. Boniface is, as far as can be seen, composed of chalk. As however this, like all other chalk hills, is in the state of a steep slope, covered with turf, perhaps by giving into its face the stony stratum might be discovered. It is also to be observed that the chalk, which is not visible above the rock at Mirables, begins to appear soon after, and grows gradually thicker as it proceeds eastward. At St. Laurence it forms a thick cap to the rocks; and at Steephill Shute, its thickness is very much increased, and soon after nothing but chalk appears in St. Boniface's hill. In what form the rock reappears at Dunnose to the east of St. Boniface, I have had no opportunity of examining.

I should not, dear Sir, trouble you with these defective observations, but that every notice, however imperfect, may be of use when connected by future observations, and that they may serve as a stimulus to other travellers, who often go over this beautiful line of country, to turn their attention to its singular natural phenomena.

I send you two specimens of the broken flints, one from Brading, the other from near Carisbrook, but the tickets are mislaid, and I am not sure which is which,

I remain, &c.

Tilney-Street, May 26, 1801.

### III.

*Account of a Machine for sweeping Chimnies, by a Blast of Air.  
In a Letter from Mr. J. C. HORNBLOWER, Engineer.*

To Mr. NICHOLSON,

DEAR SIR,

Introduction.

I SHALL be much obliged if you will allow me a page or two in your Journal for a description of a machine I have lately got up for the purpose of cleansing chimnies from soot. My

with is, if possible, to keep alive the spark that has been kindled by a truly philanthropic individual, with a view to restore a degraded class of our species to their rank in civil society. The machine, as it is constructed solely for the purpose mentioned above, is certainly novel in its intention, and without a better acquaintance with its principle and operation than the public are at present in possession of, it is not likely to gain that notice which in my judgment it is entitled to. There is a propensity in us to turn into ridicule any thing that will admit of a ridiculous construction, or afford an opportunity for the display of our wit; and such is the unfortunate circumstance of this machine, that grinning takes precedence of gravity the moment it is announced as a candidate for the gold medal at the Adelphi. However, happy for it, and thrice happy for me, there is a tribunal which can judge fairly and scientifically of its merits, for which I present it as follows.

Propensity to  
ridicule inven-  
tions.

The body of the machine A, *Plate XIII.* is made of copper, of about 3lb. to the square foot, and its contents are about three cylindrical feet. In the middle of the cover, which is soldered on, is a syringe or condenser, having its handle as appears above the cover. On one side of the cover is inserted a crooked pipe, having a valve opening inward in its inner or lower end; the stem of which comes up through the pipe, and terminates with a button C. The pipe is continued by a flexible leather one, to which is united the tin plate tube B B.

Description of  
the apparatus.  
A vessel, into  
which air is  
condensed, com-  
municates with  
a tube charged  
with small  
gravel, which  
being blown up  
the chimney  
brings down the  
soot.

This tube B B has a cross bar of tin plate fixed edge-ways in the bottom or breech of the tube which detaches from the leather pipe somewhat like a pistol barrel, leaving a piece of tube about six inches remaining to the leather pipe, into the bottom of which this case bar is fixed, and serves to receive a charge of small gravel, having a piece of paper first laid on the cross bar; the other part of the tube is then to be replaced, and the air-vessel supposed to be full; the valve is pressed down by a little lever accompanying the machine, and its contents are discharged into the shaft of the chimney; and if there is any soot worth sweeping away, it will come down.

#### *Observations concerning the Machine.*

I do not pretend to have made any new discovery in the construction or application of the machine, for it is well known that a musket has often been applied for the same purpose, and

This operation  
is similar to  
firing a musket  
up the chimney.  
with



with as good effect as need be, barring the hazard of setting fire to the foot; and indeed, when a chimney has accidentally taken fire, it is the best and most expeditious mode of putting it out, at least of bringing the foot into a situation by which it may be extinguished.

Estimate of the velocity of the extended air, and its effect in the chimney; together with that of the gravel.

I have said that the vessel contains nearly three cylindrical feet, and we cannot crowd three atmospheres of air into it, in which case there will be 42 lbs. per inch square at round numbers, pressing against the charge in the tube, or rather against the valve, the tube being  $2\frac{1}{4}$  inch diameter, which amounts to 168 lbs. for the whole area. This air will all be discharged in one second, the mean velocity of which may be fairly reckoned at 50 feet, in that time having impetus in itself sufficient to carry away any foot of consequence in a chimney of 100 feet high; but when we take into the account the charge of gravel, being alternately incident and reflected on all sides of the chimney, we need not fear to affirm that it is perfectly applicable to its intention.

Additional tubes for curved chimnies.

To render it as universal as possible, there must be another tube, to be occasionally used, when the side of the chimney near the fire place is gathered over in order to bring the throat of the flue over the fire, as at *Fig. II.* from *a* to *b*, or it should be in several pieces, to conform to the height of this gathering, and in this case the charge must be at the upper fissure at *b*, and the pipe stayed as perpendicularly as can be conveniently done; for which purpose there is a piece made to fix on the pipe, having two stems, as *Fig. III.* which if put in the bearing side of the pipe, will keep it upright and in the crater of the flue. The machine thus constructed is to be laid on a little truck, and transported from house to house, with a gallon of gravel to begin with; because until it has obtained the good opinion of the public, it would not be necessary to attempt such regulations as would afterwards be deemed requisite to facilitate the operation.

Trial at the house of the Society of Arts in the Adelphi.

This poor thing, brought forth under other disadvantages besides its singularity, was conducted to the Adelphi, to undergo an eventful trial, in competition with some others, who had no such singularity to be imputed to them; they being all descendents of the stock of one family, the remotest relation between each not being more distant than cousin-german.—

Other machines also exhibited there.

One of them however possessed great originality and ingenuity in



in the design. It consisted of a congeries of brushes, which in preparation for action were collapsed by their construction so as to admit of being thrust up the chimney with very little resistance, and when it had gained the top, on pulling a string, it expanded to the dimensions of the chimney, and brought the foot down before it; but from want of a mechanical attention to its construction, some parts being too strong, and some too slight, it broke in the first experiment.

Brushes.

Another (very elegant instrument) was presented to the committee, consisting of a large brush, such as I have seen in some churches, to sweep down the cobwebs, with a scraper attached to it; lengthened to the height of the chimney by a series of whalebone rods, whipped together as in the construction of waggoners' whips, very accurately jointed to each other. Indeed this was essential and common to them all, except one; and that was a brush, not to be sent up the chimney; but down it, by getting on the top of the house, and putting it into the chimney, and so letting it fall to the bottom. However, it was said, they would all do very well but mine, as a machine like that was adopted to a rectilinear flue only; whereas theirs had so many joints and other properties to conform to a curvature, that they would be sure to go through a crooked flue as well as a strait one.

A single brush.

Notion that the author's machine is suited only to a strait flue.

When the late Doctor Johnson had any thing advanced to him of the marvellous kind, his way of hesitation to admit it, was by saying "It may be very true, but it is impossible."—So I say with respect to the disposition of these rods, when set in competition with a volume of air going at the rate of fifty feet in a second from the lower part of the chimney to the top. What is there to stop it? What is there to prevent its conformity to the curvature or angles, if you will?

The same denied.

The manner in which these brushes act, let their construction be what it may, is by striking off the foot. What does a boy within the flue with the brush in his hand? To be sure sweeping is the most appropriate term for what he does; but then, unless, we attend to the peculiarity of the action, and consider how far it is possible to effect that sweeping by another mode of action, we shall not get any further than the boy in the flue. But is it not as easy to conceive of a quantity of air being put into action along a road or a street, so as to sweep away the dust before it? No; you will say, it blows it along; but

To blow down the foot is as natural as to sweep it down.

but the term blowing associates only with the accustomed notion of the action of wind, and was the effect to be produced by the agency of steam; the term blowing would more easily be dismissed, and we might call the action *driving* it along.

The term sweeping is in some instances used to express the action of wind.

When we associate this agency of the wind with its effect on a field of ripening corn, we conceive of it *sweeping* along or over the corn; and the same effect would follow, were we to conceive of a large brush of long hair carried on the tops of corn; so that however popular expression, or the prejudice of custom may operate to distinguish the causes producing the same effect; it becomes us to waive such distinctions, and admit the efficiency of the one as much as the other.

Instances of the action of wind in a chimney.

I have observed that when a door has been suddenly opened or shut, it has brought down soot in a chimney which has a sluggish draft; or a piece of paper accidentally thrown on the fire has had the like effect; and this by a momentary acceleration of the current of air in the flue. What then will not such a machine as this perform, with such a volume of air, with such a velocity.

The air machine sweeps chimnies of all heights, and is durable.

One circumstance presents itself, by which the air machine must have a decided preference over the brushes; which is this, you must know the height of the chimney, and adapt the length of the rods to that height, or else you will not know when you are in the flue or out of it. Whereas my machine knows no necessity for such a punctilio; all that is necessary being only to give so many shocks of the condenser to a chimney of two stories, and so many to one of three, and so on. And again, these brushes in the very outset of trial made with them, are subject to accidents, and will most assuredly wear out very fast. What must become of those whalebone rods when the sewing is rubbed through in passing up and down against the projections of uncereemonious bricks and mortar? What repairs will they not be subject to in the course of one day's action? Whereas the air machine will sweep a hundred chimnies, and be repaired for two-pence; it wanting only a little oil in the condenser.

It was tried at the Adelphi upon chimnies that contained no soot.

When this machine was carried to the Adelphi, it came to be the third in course for the trial, and it was ordered *down*, *down* to the foundation of the house, for fear a chimney should not be found high enough in any other part of the building.— But when it was brought, behold there had been two sweepers already, which obliged me to apologize for the machine, by observing



observing that they had taken down all the foot, and left nothing for it to do \*. Upon which a suggestion was made, that there was another chimney which had a fire alternately during the winter †; and on examination it was agreed, that, by the appearance of the foot about the breast of the flue, the chimney was very foul, of which, however, I had my doubts. But it was thought expedient to try that chimney; and the first charge we gave it brought no foot. We gave it another charge, but no foot came; and it was thought best to send up a boy, with proper implements, to sweep the chimney; but no foot came down, except what may be said to be brought away by violence, and did not exceed a quart-potful, and with it much mortar or pargetting.

This brings me to some observations I have made on the condition of chimnies in general, as to the foot they may contain.

I tried this machine at Shoreditch Workhouse, where the chimnies are very lofty, for which reason I preferred them; but when I came to see their circumstances I suspected how it was, and we tried two of them where the fires had been very economical, and may continue so winter after winter, without ever requiring to be swept at all, for reasons I will give by and by. I then went to a neighbour of mine, and we swept two of his chimnies; one had no foot, the other had a good deal; and we thought fit to get a boy to go up the last chimney, and he brought down about a hatful of foot; however, my neighbour was satisfied, and signed my certificate to the Committee at the Adelphi.

*Trials of the machine with other chimnies.*

We then went to another neighbour's, and swept his parlour chimney, and dislodged a great quantity of foot. We were to have swept his kitchen chimney, but it having a smoke-jack, we could not apply the machine for want of the additional pipe in *Fig. 2*.

\* It appears the committee had no regard to the effect of any of the candidate's inventions, any further than a fair thrust up the chimney.

† With another in the same room I suppose.



## IV.

*Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double Stars; with an Investigation of the Cause to which they are owing. By WILLIAM HERSCHEL, LL. D. F. R. S. From the Philosophical Transactions for 1803.*

(Concluded from Page 227.)

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

I SHALL now enter into a more detailed examination of the several angles of position I have taken at different times, and show that they agree perfectly well with the appearances which must arise from the revolution of a small star round Castor. A calculation of these angles may be had, by finding the annual motion of the small star, from the change of  $21^{\circ} 54'$ , which has been shown to have taken place in 23 years and 142 days. Accordingly, I have given, in the first column of the following table, the time when the angles were taken. In the second, are the angles as they were found by measure; they are all in the north-preceding quadrant. The third column contains a calculation from the annual motion of  $56', 18$ , obtained as before mentioned: it shows what these angles should have been, according to our present supposition of a revolving star. And the last column gives the difference between the observed and calculated angles.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Nov. 5, 1779 -	$32^{\circ} 47'$	$32^{\circ} 47'$	$0^{\circ} 0'$
Feb. 23, 1791 -	22 57	22 11	+ 0 46
Feb. 26, 1792 -	27 16	21 16	+ 6 0
Dec. 15, 1795 -	13 52	17 42	- 3 50
March 26, 1800	18 8	13 41	+ 4 27
April 23, 1800	10 30	13 37	- 3 7
Dec. 31, 1801 -	7 58	12 2	- 4 4
Jan. 10, 1802 -	10 53	12 1	- 1 8
Jan. 23, 1802 -	10 28	11 59	- 1 31
Feb. 28, 1802 -	13 0	11 53	+ 1 7
Feb. 11, 1803 -	7 53	11 0	- 3 7
March 23, 1803	13 23	10 54	+ 2 29
March 27, 1803	10 53	10 53	0 0

On

On looking over the fourth column of this table, it will be found, that the differences between the observed and calculated angles are not greater than may be expected, considering that most of the early measures are single, and cannot have the accuracy which may be obtained by repetition. Even as they are, we must acknowledge them sufficient to ascertain the gradual change in the angle of position of the two stars. In one place, the difference amounts to six degrees; but it will soon appear, that a more accurate annual motion gives a calculated position which takes off much of the error of this measure.

Observations and inferences respecting the changes of relative situation in stars extremely near each others

In a conversation with my highly esteemed friend the Astronomer Royal, he happened some time ago accidentally to mention, that Dr. Bradley had formerly observed the two stars of  $\alpha$  Geminorum to stand in the same direction with Castor and Pollux. It occurred to me immediately, that if the time of this observation could be nearly ascertained, it would be of the greatest importance to the subject at present under consideration. For, should Dr. Bradley's position be very different from a calculated one, it would induce us at once to give up the idea of a revolving star. The observation was made by Dr. Bradley with a view to see whether any change could be perceived in the course of the year, by which the annual parallax of the stars might be discovered. Dr. Maskelyne, who had this information from Dr. Bradley in conversation, had made a memorandum of it in his papers. He has been so kind as to look for it; and, as soon as he found the note, he sent me the following copy, which I have his permission to transcribe.

*“Double star Castor. No change of position in the two stars: the line joining them, at all times of the year, parallel to the line joining Castor and Pollux in the heavens, seen by the naked eye.”*

Dr. Maskelyne informs me, that the observation must have been made about the year 1759; and also mentions, that he himself verified the fact, as to the line joining the two stars appearing through the telescope parallel to the line joining Castor and Pollux, in 1760 or 1761; but that he did not examine it at various times of the year.

The advantage of having an angle of position observed in 1759 by Dr. Bradley, and so soon after verified by Dr. Maskelyne, will give us an addition of 20 years to our period. On calculating the right ascension and polar distance of Castor and

Pollux

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

Pollux for November 5, 1759, it appears, that a line drawn from Pollux through Castor, must have made an angle of  $56^{\circ} 32'$  north preceding with the parallel of that star; and, this being also the position of our double star, we have an interval of 43 years and 142 days, for a change of  $45^{\circ} 39'$ , from the time of Dr. Bradley's observation to that of my last measure of the angle. By this we are now enabled to correct our former calculation, which was founded upon a supposition that the first angle of position I had taken was perfect; but this could hardly be expected, and on examination it appears that the measure was  $2^{\circ} 40'$  too little. The annual motion, by our increased period, is  $1^{\circ} 3',1$ ; and the computation of the angles of position in the third column of the following table, as well as the differences contained in the fourth, are made according to this motion.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Nov. 5, 1759 -	$56^{\circ} 32'$	$56^{\circ} 32'$	$0^{\circ} 0'$
Nov. 5, 1779 -	32 47	35 29	- 2 42
Feb. 23, 1791 -	22 57	23 36	- 0 39
Feb. 26, 1792 -	27 16	22 32	+ 4 44
Dec. 15, 1795 -	13 52	18 32	- 4 40
March 26, 1800	18 8	14 3	+ 4 5
April 23, 1800	10 30	13 58	- 3 28
Dec. 31, 1801 -	7 58	12 12	- 4 14
Jan. 10, 1802 -	10 53	12 10	- 1 17
Jan. 23, 1802 -	10 28	12 7	- 1 39
Feb. 28, 1802 -	13 0	12 1	+ 0 59
Feb. 11, 1803 -	7 53	11 1	- 3 8
March 23, 1803	13 23	10 54	+ 2 29
March 27, 1803	10 53	10 53	0 0

When the result of this table is compared with that of the former, it will be seen that my observations agree not only very well with Dr. Bradley's position, but even give more equally divided differences than before, so that the excess and differences counteract each other better than in the first table.

The time of a periodical revolution may now be calculated from the arch of  $45^{\circ} 39'$ , which has been described in 43 years and 142 days. The regularity of the motion gives us great reason to conclude, that the orbit in which the small star moves about



about Castor, or rather, the orbits in which they both move round their common centre of gravity, are nearly circular, and at right angles to the line in which we see them. If this should be nearly true, it follows, that the time of a whole apparent revolution of the small star round Castor, will be about 342 years and two months.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

*γ Leonis.*

Our foregoing discussions will greatly abridge the arguments which may be used, to shew that this star and its small companion are also probably united in forming a binary system. But, in order to give more clearness to our disquisition, we shall follow the arrangement which has been used with  $\alpha$  Geminorum, and prefix the same letters to our paragraphs. Then, if any one article should appear to be not sufficiently explained, we need but turn back to our first double star, where the same letter will point out what has already been said more at large on the subject; and an application of it may easily be made.

The distance of the stars  $\gamma$  and  $x$ , as I shall again call the small one, has undergone a visible alteration in the last 21 years. The result of a great number of observations on the vacancy between the two stars, made with the magnifying powers of 278, 460, 657, 840, 932, 1504, 2010, 2589, 3168, 4294, 5489, and 6652, is, that with the standard power and aperture of the 7-feet telescope, the interval in 1782 was  $\frac{1}{4}$  of a diameter of the small star, and is now  $\frac{3}{4}$ . With the same telescope, and a power of 2010, it was formerly  $\frac{1}{2}$  of a diameter of the small star, and is now full 1 diameter. In the years 1795, 1796, and 1798, the interval was found to have gradually increased; and all observations conspire to prove, that the stars are now  $\frac{1}{2}$  a diameter of the small one farther asunder than they were formerly. The proportion of the diameter of  $\gamma$  to that of  $x$ , I have, by many observations, estimated as 5 to 4.

The first measured angle in 1782, is  $7^{\circ} 37'$  north following\*; and the last, which has been lately taken, is  $6^{\circ} 21'$  south fol-

\* In my second Catalogue of double Stars, (Phil. Trans. for 1785, page 48,) the angle of position is  $5^{\circ} 24'$ . This was taken April 18, 1783; and, not being acquainted with the motion of the small star, I supposed it to be more accurate than the former measure.

lowing.

Observations and inferences respecting the change of relative situation in stars extremely near each other. lowing. The sum of these angles gives  $13^{\circ} 58'$ , for the change that has taken place in 21 years and 38 days. To account for this, we are to have recourse, as before, to the various motions of the three bodies.

#### *Single Motions.*

(a) The motion of  $x$  alone cannot be admitted, since it is known that  $\gamma$  Leonis is not at rest. The annual proper motion of this star, according to M. De la Lande, is  $+0'',38$  in right ascension, and  $0'',04$  in declination towards the south.

(b)  $\gamma$  cannot be the only moving body; because its motion in right ascension only, which, in 21,1 years, at the parallel of  $\gamma$ , amounts to  $7'',49$ , would have long ago taken it away from the small star.

(c, d, e,) The sun cannot be the only moving body; because its motion in right ascension will not account for that of  $\gamma$  Leonis, which star therefore cannot be at rest. And, if we were willing to give up the former assumed solar motion, in order to fix upon such a one as would explain the motion of  $\gamma$ , we should be under a necessity to contradict the united evidence of the proper motions of many principal stars which are in opposition to it.

#### *Double Motions.*

(f) When two motions are proposed, we cannot fix upon  $\gamma$  and  $x$  for the moving bodies, unless we should set aside the solar motion, and this, we know, cannot properly admit of a doubt.

(g) That we cannot allow O and  $x$  to be the two bodies in motion, follows from the insufficiency of the solar motion to account for that of  $\gamma$ , which must be real, or at least partly so.

(h) If O and  $\gamma$  are the moving bodies, the given situations of  $x$ , in the years 1782 and 1783, point out an apparent motion of  $x$ , which must be intirely owing to the solar parallax; and, therefore, those who will admit this hypothesis, must grant the discovery of the motion of the solar system, and of the proportional parallax of the two stars  $\gamma$  and  $x$ . Let us however examine whether any motion of the sun, such as we can admit, will account for the change of position and distance pointed out by my observations of the small star near  $\gamma$  Leonis.

The

The joint effect of proper motion and parallax, has carried  $\gamma$  from its situation in 1782 to that where we now find it. The small star, having all this time, in appearance, accompanied  $\gamma$ , must have gone through a space of  $7''.98$ , in a direction which makes an angle of  $8^\circ 30'$  south following with the parallel of  $\gamma$ , in order to be at its present distance from it, and at the same time to have undergone the required change of its angle of position. Now, as the supposition we are examining requires this small star to be actually at rest, it will be necessary to assign to the sun an opposite motion of the same velocity, in order to make that of  $x$  only an apparent one. The consequence of this will be a retrograde motion of the sun, which it is well known cannot be admitted.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

### *Motion of the three Bodies.*

(2) A motion of all the three bodies, is the only way left to explain the phenomena of our double star; and I shall now again point out the very particular circumstances which it is requisite should all happen together, to produce the intended effect.

Let the motion of the sun, with the same annual velocity 1, as in the case of  $\alpha$  Geminorum, be directed towards  $\lambda$  Herculis. Then the effect of this motion will shew itself at the place of  $\gamma$  Leonis, in the annual velocity of ,3314, and in a direction which makes an angle of  $31^\circ 11'$  south preceding with the parallel of that star. In this calculation, I have admitted the distance of the largest of the two stars of  $\gamma$  from the sun to be 3, that of  $\alpha$  Geminorum being 2. But, if any other distance should hereafter be considered as more probable, the calculation may be easily adapted to it. The consequence of the parallax thus produced on  $\gamma$  Leonis in 21,1 years, will be an apparent motion of  $2''.788$  south preceding, in the abovementioned direction; and, on  $x$ , it will be in the same time, and in the same direction,  $1''.091$ . As the small star must not be too near  $\gamma$ , we have, in the calculation, supposed it to be at the distance of 4 from O.

The real annual proper motion of  $\gamma$  is required to be 3,5202; and its direction must make an angle of  $3^\circ 40'$  north following with the parallel. By this motion alone,  $\gamma$  would have passed over a space of  $9''.87$  in 21,1 years; but, when it is combined with the apparent motion arising from parallax, the star will come into its present situation.



Observations and inferences respecting the changes of relative situation in stars extremely near each other.

The real annual motion of  $x$  must be 4,6294, in a direction  $0^{\circ} 20'$  south following. This will carry it over  $9''.74$ , in 21,1 years; and, when combined with the apparent motion which the solar parallax will occasion, both together will bring it to its proper distance from  $\gamma$  Leonis, and to a situation which will agree with the last observed angle of position.

From what has been said, it is again evident, that not only as many particular circumstances must concur in explaining the phenomena of  $\gamma$  Leonis as we have pointed out with  $\alpha$  Geminorum, but that a very marked condition is added in our second double star, which requires an adjustment of velocities in  $\gamma$  and  $x$ , which shall also fit the same solar motion that was used in  $\alpha$  Geminorum. And this proves, that every additional double star which requires the same condition in order to have its appearances explained, will enforce the arguments which have been used, in a compound ratio.

If, on the other hand, we have recourse to the simplicity of the known effects of attraction, and admit the two stars of our present double star to be united in one system, all the foregoing difficulties of accounting for the observed phenomena will vanish. Whatever may be the proper motion of the sun, the parallax arising from that cause will affect both stars equally, on account of their equal distance from the sun. The proper motion of  $\gamma$  Leonis also may be in any direction, and of any given velocity, such as will agree best with astronomical observations; since the motion of a system of bodies will not interfere with the particular motion of the bodies that belong to it, so that our secondary star will continue its revolution round the primary one without disturbance.

It will now be necessary to examine the observed angles of position, and to compare them with calculated ones; but, as there has been a change in the distance of the two stars, it is evident that, if they revolve in circular orbits, the situation of the plane of their revolution must be considerably inclined to the line in which we see the principal star.

Let N F S P, Fig. 2, be the orbit in which  $x$  revolves about  $\gamma$  placed in the centre. Suppose a perpendicular to be erected at  $\gamma$  leading to O, not expressed in the figure. By an observation of Feb. 16, 1782, we have the angle  $F \gamma x = 7^{\circ} 37'$  north following; and the proportion of the apparent diameter of  $\gamma$  to that of  $x$  has been given as 5 to 4. It has also been ascer-

tained

tained, that the vacancy between the apparent diameters, when the first angle of position was taken, was  $\frac{1}{4}$  diameter of the small star; and the last angle of position being  $6^{\circ} 21'$  south following, with a distance between the stars of  $\frac{3}{4}$  diameter of the small star, we obtain the two points or centres of the small stars  $xx'$ , through which an ellipsis  $abxx'cd$  may be drawn about  $\gamma$ . This will be the apparent orbit in which the small star will be seen to move about  $\gamma$ , by an eye placed at  $O$ . And the inclination of the orbit to the line in which we see the double star, will be had sufficiently accurate to enable us to give a calculation of the several angles of position that have been taken. The ellipsis we have delineated shews that the small star, in its first situation  $x$ , could not be much past its conjunction at  $b$ , and that, consequently, in passing from  $x$  to  $x'$ , the parts of the apparent elliptical arch, which are projections of the real circular arch  $hh'$ , would be described in times nearly proportional to the time in which the whole arch has been described. Upon these principles, the third column of the following table has been calculated.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Feb. 16, 1782 -	$7^{\circ} 37' nf$	$7^{\circ} 37'$	$0^{\circ} 0'$
April 18, 1783 -	$5 24 nf$	$6 51$	$- 1 27$
Jan. 24, 1800 -	$3 16 sf$	$4 15$	$- 0 59$
Feb. 19, 1800 -	$3 23 sf$	$4 18$	$- 0 55$
March 26, 1800	$3 47 sf$	$4 22$	$- 0 35$
Jan. 26, 1802 -	$6 4 sf$	$5 35$	$+ 0 29$
Feb. 10, 1803 -	$3 33 sf$	$6 16$	$- 2 43$
March 22, 1803	$6 32 sf$	$5 20$	$+ 0 12$
March 26, 1803	$6 21 sf$	$6 21$	$0 0$

The difference between the calculated and observed angles, contained in the fourth column of the preceding table, is so little, that we may look upon the gradual change of these angles as established by observation; and we may form a calculated estimate of the time which will be taken up by the mutual revolution of the two stars. The apparent places  $xx'$ , being referred to their real ones, give the arch  $hh'$ , which has been described in 21 years and 38 days; and this arch, seen from the centre  $\gamma$ , is about  $6^{\circ} 20'$ : it follows, that the length of a whole revolution of our small star round  $\gamma$  Leonis, will be about 1200 years.

*ε Bootis.*

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

This beautiful double star, on account of the different colours of the stars of which it is composed, has much the appearance of a planet and its satellite, both shining with innate but differently coloured light.

There has been a very gradual change in the distance of the two stars; and the result of more than 120 observations, with different powers, is, that with the standard magnifier, 460, and the aperture of 6.3 inches, the vacancy between the two stars, in the year 1781, was  $1\frac{1}{2}$  diameter of the large star, and that it now is  $1\frac{3}{4}$ . By some earlier observations, the vacancy was found to be considerably less in 1779 and 1780; but the 7-foot mirror then in use was not so perfect as it should have been, for the purpose of such delicate observations. By many estimations of the apparent size of the stars, I have fixed the proportion of the diameter of  $\epsilon$  to that of  $\alpha$ , as 3 to 2. August 31, 1780, the first angle of position measured  $32^{\circ} 19'$  north preceding\*; and, March 16, 1803, I found it  $44^{\circ} 52'$ , also north preceding: the motion, therefore, in 22 years and 207 days, is  $12^{\circ} 33'$ . It should also be noticed, that while the apparent motion of  $\alpha$  Geminorum, and of  $\gamma$  Leonis, is retrograde, that of  $\epsilon$  Bootis is direct.

A proper motion in this star, if it has any, is still unknown; our former arguments, therefore, cannot be applied to it, without some additional considerations; and, as many others of my double stars will stand in the same predicament, I shall give an outline of what may be said, to show that this, and probably many of the rest, are also binary systems.

*Single Motions.*

(*a—c*) If  $\epsilon$  Bootis is a star in which no proper motion can be perceived, we may infer, from the highly probable motion of the solar system, that this star, which is of the third magnitude, and on that account within the reach of parallax, must have a real motion, to keep up with the sun, in order to prevent an

\* The angle of position, in my first Catalogue of double Stars, Phil. Trans. for 1782, page 115, is  $31^{\circ} 34'$  (it should be  $54'$ ) north preceding. This will be found to be a mean of the three first measures hereafter given in a table of positions.



apparent change of place, which must otherwise have happened. In this case, no single motion can be admitted to explain the phenomena of our double star. But, if a real proper motion of  $\epsilon$  Bootis should hereafter be ascertained, the arguments we have used in the case of  $\gamma$  Leonis, will lead to the same conclusion.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

### *Double Motions.*

(f)  $\epsilon$  and  $x$  cannot be the moving bodies; and our former argument (f) will apply to every double star whatsoever.

(g) O and  $x$  cannot be alone in motion; for, if no motion in  $\epsilon$  can be perceived, it must move in a similar manner with the sun, and none of the three bodies will be at rest. But, if its proper motion shall hereafter be found out, it must either be exactly the reverse of the solar motion, and therefore only an apparent one; or it will be more or less different. In the latter case, all the three bodies must be in motion; in the former, the exact quantity of the solar motion will be discovered, and the relative parallax of many stars may be had by observation.

(h) If O and  $\epsilon$  are the two bodies in motion, and if at the same time no motion in  $\epsilon$  can be perceived, then the apparent motion of  $x$  must be intirely owing to the different effect of the solar parallax on  $\epsilon$  and  $x$ ; but the effect of the solar parallax on  $x$ , can only be in a direction contrary to the motion of the sun, which, being north following the small star, whether it be nearer or farther from us than  $\epsilon$ , must have an apparent motion towards the south preceding part of the heavens. But this is directly in opposition to my observation of the motion of the small star, which, these last 23 years, has been directed towards the north following.

### *Motion of the three Bodies.*

(i) Let the motion of the sun be again towards  $\lambda$  Herculis; then, if no motion in  $\epsilon$  Bootis be perceivable, it must move exactly like O. Highly improbable as it is, let it be admitted.

Then, in addition to this extraordinary supposition, a third motion is also required for  $x$ , which, aided by the solar parallax, is to carry it likewise within a quarter of a diameter of  $\epsilon$ , into the same place where, though unperceived, the large star has been carried by its own motion; that is, in order to be apparently

rently

Observations and inferences respecting the change of relative situation in stars extremely near each other.

rently at rest, the sun,  $\epsilon$  Bootis, and its small companion, must all move exactly alike, setting aside the very little difference in the position and distance of the small star, which, in the whole, amounts to little more than 6-tenths of a second; than which, certainly nothing can be more improbable.

But, if  $\epsilon$  shall hereafter be found not to have been at rest during the time of my observations upon it, then its place will be given; and, since also the situation of  $x$ , with respect to  $\epsilon$ , is to be had from my angles of position and distances of the two stars, the case will be similar to that which has already been considered, in the paragraph (i), under the head of  $\gamma$  Leonis.

I may here add a remark with regard to  $\epsilon$  Bootis, which will be applicable to several more of my double stars. In the milky-way, a multitude of small stars are profusely scattered, and their arrangement is very different from what we perceive in those parts of the heavens which are at a considerable distance from it. About  $\epsilon$  Bootis, which is situated in what I have formerly called figuratively a nebulous part of the heavens\*, there are, comparatively speaking, hardly any stars; and, that so remarkable a star as  $\epsilon$  should have a companion, seems almost to amount to a proof that this very companion is, as it appears to be, a connected star. The *onus probandi*, therefore, ought in justice to fall to the share of those who would deny the truth of what we may call a fact; and I believe the utmost they could do, would be to prove that we may be deceived; but they cannot show that this star has no connection with  $\epsilon$  Bootis.

This argument will be much supported, when we consider that many of the double stars in the milky-way are probably such as have one of the scattered stars, nearly in the same line, at a great distance behind them. In this case, the two stars of the double star have no connection with each other; and the great number of them in the milky-way, is itself an indication of this effect of the scattered multitude of small stars. In the single constellation of Orion, for instance, we have no less than 43, pointed out by my catalogues; ten of which are of the first class, and yet have undergone no change of distance or position since I first perceived them. But, with apparently insulated stars, such as  $\epsilon$  Bootis, the case is just the reverse.

\* See Phil. Transf. for 1784, page 449.

If, in consequence of our former arguments, and the present Observations and remarks, we place  $\epsilon$  Bootis among the stars which hold a smaller inferences respecting the one in combination, we may delineate its orbit as in Plate VIII. change of relative situation in stars extremely near each other.

Fig. 3.  
Let P N F S represent a circle, projected into the elliptical orbit  $axx'bcd$ .  $\epsilon$  is the large star; and  $x x'$  are the first and last measured north preceding situations of the small one, as given in the following table.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
August 31, 1780	32° 19'	33° 58'	— 1° 39'
March 13, 1781	30 21	34 13	— 3 52
May 10, 1781 -	33 1	34 18	— 1 17
Feb. 17, 1782 -	38 26	34 40	+ 3 46
August 18, 1796	45 32	41 40	+ 3 52
Jan. 28, 1802 -	49 18	44 19	+ 4 59
August 31, 1802	46 47	44 36	+ 2 11
March 23, 1803	43 43	44 52	— 1 9
March 26, 1803	44 52	44 52	0 0

The real motion from  $h$  to  $h'$  is projected into that from  $x$  to  $x'$ ; and, while the elliptical arch subtends an angle of  $12^\circ 33'$ , the circular one will be about  $4^\circ 50'$ .

From the figure of the orbit, we may conclude that the small star, in its first position, at  $x$  or  $h$ , was not more than between 30 and 40 years past its conjunction; and that, consequently, the parts of the arch  $xx'$ , were nearly proportional to the times of their being described. The positions have been calculated upon this principle; but with some allowance for the first observed angle, which I suppose to have been a little too small; and, though the differences of the observed and calculated angles are pretty considerable, the observations are still sufficiently consistent to prove the gradual change of the situation of the small star.

The quantity of the change in 22 years and 207 days, will show that a periodical revolution cannot take up less than 1681 years. The real figure and situation of the orbit, with many other particulars, are still unknown; it is, therefore, unnecessary to point out the uncertainties in which the investigation of the periodical time of the small star about  $\epsilon$  Bootis must long remain involved.

$\zeta$  Herculis.



*ζ Herculis.*

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tive situation in  
stars extremely  
near each other.

My observations of this star furnish us with a phenomenon which is new in astronomy; it is, the occultation of one star by another. This epoch, whatever be the cause of it, will be equally remarkable, whether owing to solar parallax, proper motion, or motion in an orbit whose plane is nearly coincident with the visual ray. My first view of this star, as being double, was July 18, 1782. With 460, the stars were then  $\frac{1}{2}$  diameter of the small star asunder. The large star is of a beautiful bluish white; and the small one ash-coloured.

July 21, of the same year, I measured the angle of position,  $20^{\circ} 42'$  north following. With the standard power, the distance of the stars remained as before. With 987, they were one full diameter of the small one asunder.

In the year 1795, I found it difficult to perceive the small star; however, in October of the same year, I saw it plainly double, with 460; and its position was north following.

Other business prevented my attending to this star till the year 1802, when I could no longer perceive the small star. Sometimes, however, I suspected it to be still partly visible; and, in September of the same year, with 460, the night being very clear, the apparent disk of *ζ Herculis* seemed to be a little lengthened one way. With the ten-feet telescope, and a power of 600, I saw the two stars of *η Coronæ* very distinctly; and, having in this manner proved the instrument to act well, I directed it to *ζ Herculis*, and found it to have the appearance of a lengthened, or rather wedge-formed star; after which, I took a measure of the position of the wedge.

Our temperature is seldom uniform enough to permit the use of very high powers; however, on the 11th of April, 1803, I examined the apparent disk, with a magnifier of 2140, and found it, as before, a little distorted; but there could not be more than about  $\frac{1}{8}$  of the apparent diameter of the small star wanting to a complete occultation. Most probably, the path of the motion is not quite central; if so, the disk will remain a little distorted, during the whole time of the conjunction. Our present observations cannot determine which of the stars is at the greatest distance; but this will occasion no difference in the appearance; for, if the small star should be the nearest, its light will be equally lost in the brightness of the large one.

The observations I have made on this star, are not sufficient to direct us in the investigation of the nature of the motion by which this change is occasioned.

We may however be certain, that with regard to

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

### *Single Motions.*

(a, b) Neither  $x$  nor  $\zeta$  can be supposed to be the only moving bodies, without contradicting the highly probable arguments for the sun's motion.

(c, d) If we admit the sun to be the moving body, the stars  $\zeta$  and  $x$  being at rest, we may calculate the effect of the solar parallax upon them, as follows. Let  $O$  move towards  $\lambda$  Herculis, with the annual velocity 1, as in the case of  $\alpha$  Geminorum; then, from the situation and magnitude of the large star of  $\zeta$  Herculis, which we will suppose 4 $m$ , the effect of the solar motion at  $\zeta$  will be only ,0522; and, at  $x$ , supposed to be at the distance 5 $m$ , it will be ,0418. This will show itself at the parallel of  $\zeta$  in a direction of  $25^{\circ} 5'$  north preceding, the solar motion being in the opposite direction south following. But this parallax will only produce, in 20 years and 10 months, an apparent change of  $0'' ,444$  in  $\zeta$ , and of  $0'' ,355$  in  $x$ ; and will separate the stars, instead of bringing them to a conjunction.

(e) A considerable advantage may be gained, by placing  $x$  at a little more than  $\frac{1}{3}$  the distance of  $\zeta$  from  $O$ . For as, in the above-mentioned time, this would make the effect of parallax upon it  $1'' ,18$ , a conjunction should now take place. But then the stars, though very near each other, would not be quite in contact; much less could one of them occasion an occultation of the other. The supposition also, that the small star should be only  $\frac{1}{3}$  of the distance of the large one from us, is not very favourable to the hypothesis.

### *$\delta$ Serpentis.*

This double star has undergone a very considerable change in the angle of position, but none in the distance of the two stars. The 5th of September, 1782, an accurate measure of the position was  $42^{\circ} 48'$  south preceding; and February 7, 1802, it measured  $61^{\circ} 27'$  south preceding. In 19 years and 155 days, therefore, the small star has moved, in a retrograde order, over an arch of  $18^{\circ} 39'$ .

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Every argument, to examine the cause of this motion, which has been used with  $\epsilon$  Bootis, in the paragraphs from (a) to (i), will completely apply to this star; from this we may conclude, that the most natural way of accounting for the observed changes, is to admit the two stars to form a binary system. In this case we calculate, with considerable probability, that the periodical time of a revolution of the small star round  $\delta$  Serpentis, must be about 375 years.

### *$\gamma$ Virginis.*

This double star, which has long been known to astronomers,\* has undergone a visible change since the year 1780, when I first began my observations of it. The 21st of November, 1781, I measured the position of the two stars, which was  $40^{\circ} 44'$  south following. The stars are so nearly equal, that I have but lately ascertained the following one to be rather larger than its companion; the position, therefore, ought now to be called north preceding. By a mean of three measures, that were taken on the 15th of April, 1803, the angle was  $30^{\circ} 20' np$ .

The distance, as far as estimations by the diameter can determine, when the stars are so far asunder as these are, remains without alteration. May 21, 1781, they were  $2\frac{1}{2}$  diameters asunder; and, by estimations lately made, with the same instrument and power as were used 21 years ago, the stars are still at the same distance of  $2\frac{1}{2}$  diameters.

A very small proper motion in declination, of  $0''.02$  towards the south, has been assigned to this double star;† but the quantity is hardly sufficient for us to rely much upon the accuracy of the determination. I shall therefore rather consider  $\gamma$  Virginis as one of the stars of which we have no proper motion ascertained; and the arguments to which I shall refer, will consequently be those which have been given with  $\epsilon$  Bootis.

The change of the angle of position, in the time of 21 years and 145 days, amounts to  $10^{\circ} 24'$ ; from which we obtain the annual motion of  $29'.16$ . The observed and calculated angles, with their differences, on which it will not be necessary to make any remarks, are in the following Table.

\* *Memoires de l'Academie des Sciences.* Ann. 1720.

† *Connoissance des Temps*, Année VI. page 213.



Times of the Observations.	Observed angles.	Calculated angles.	Differences.
Nov. 21, 1781 -	40° 44'	40° 44'	0° 0'
Jan. 29, 1802 -	28 22	30 51	-2 29
April 15, 1803 -	30 20	30 20	0 0
May 28, 1803 -	32 2	30 17	+1 45

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

As a confirmation of the accuracy of these observations, we may have recourse to a position of the same stars, deduced from the places of them, as they are given in Mayer's Zodiacal Catalogue. By two observations reduced to the beginning of the year 1756, the preceding one was 3'',8 before the other in right ascension, and 5'',3 more north than that star. From this we calculate the position, which was 54° 21' 37" north preceding. The interval from the 1st of January, 1756, to the 21st of November, 1781, is 25 years and 325 days. When this is added to the period I have given, we have 47 years and 105 days, for a motion of 24° 2'. The annual motion, deduced from this lengthened period, which is 30',5, differs less than 1½ minute from that which has been calculated from my observations. With the assistance, therefore, of Mayer's observations, which greatly supports our calculation, we may conclude, that the two stars of  $\gamma$  Virginis revolve round each other in about 708 years.

## V.

*Account of the Invention of the Compound-Barrel and Winch, and its useful Application on a Large Scale. In a Letter from Mr. J. J. HAWKINS.*

To Mr. NICHOLSON.

SIR,

IN No. 25 of your useful Journal, p. 50, you mention the compound-barrel and winch, as the invention of the celebrated Mr. George Eckhardt.

Not in the least wishing to subtract from the well known inventive talents of that Gentleman, I beg leave to say, that, if it was invented by him, it was also invented to my certain knowledge, by the late Mr. Robert Mc Kean of Philadelphia, son to the present governor of Pennsylvania.

The compound barrel and winch was also invented by Mr. R. Mc Kean.

**In 1795 or 1796.** Mr. Mc Kean communicated the idea to me a few days after it first occurred to him, in the year 1795 or 1796; his plan differed, however, in two points, from that described in your Journal; for, firstly, he made use of handspikes applied into holes in the end of the largest barrel, whenever the winch did not afford him sufficient power; and, secondly, when he found it necessary to raise any thing above the level of his windlafs, he added two other pullies for the rope to pass over.

**Dimensions and application of a compound barrel.**

About the year 1797, Mr. Mc Kean erected a saw-mill worked by a steam engine, at Bordenton in New Jersey; where he employed this crane to raise his logs, from the ground to the frame on which they were to be cut, being a height of about twelve feet; the circumference of the largest barrel was about six feet, and its length three feet; the smallest barrel was of the same length, and its circumference three feet, making the difference of their circumferences three feet; and, consequently one turn of the barrels, raised the log one foot six inches; and eight turns only were sufficient, to elevate it the height required.

**Its power as one to twenty.**

This windlafs was turned with handspikes six feet long; which, at the place where the power is generally applied, would describe a circle of about 30 feet; so that the power obtained, is as one to twenty. The length of rope necessary, including that which passes round the pullies, and from thence to the barrels, is about 55 feet.

**A common tackle would have required five times as much rope;**

**but thinner:**

Now in order to raise a weight 12 feet high, with a power of one to twenty, by means of the common block pullies, no less than 270 feet of rope is necessary; there is, however, this difference in the sizes of the ropes, that, in the former case supporting one half of the weight to be raised, whereas in the latter it has only to sustain one twentieth part of it.

**This windlafs does not run down.**

The windlafs has the peculiar property of holding the weight at any part of its rise or fall, without needing a ratchet-wheel and catch; by reason of the two parts of the rope pulling on opposite sides of the barrels; and although one pulls on a longer lever than the other, yet it is not sufficient to overcome the friction on the axis, which must of necessity be large, in order to give the requisite strength to the machine.

**It promises to be extensively useful.**

I have lately introduced a small windlafs of this kind, into a piece of machinery requiring alternate elevation and depression,

pression; and find it extremely convenient; and I have no doubt it will be adopted in a great variety of machinery when its valuable properties are generally known.

I am,

Sir, Yours, &c.

J. J. HAWKINS.

11, New Life Street,

March 17, 1804.

## VI.

*Proposal for constructing a Galvanic Apparatus of Great Power, for the Combustion of Metals. By a Correspondent. I. R. I.*

To Mr. NICHOLSON.

SIR,

THE following idea of a mode of increasing to an immense degree the galvanic power of inflaming metals, took its origin from Mr. Wilkinson's paper on that subject in your last Journal; and I beg that if you think this communication can be of the smallest use to that ingenious gentleman, that you will send it to him before publication.

I can hardly think that it could have escaped his penetration, (though I cannot account for his omitting the observation) that the natural consequence of his discovery relative to the ratio by which the galvanic trough increases in its power of inflaming metals, is, that the maximum of effect will be produced (from a given surface,) by one pair only; so that the same surface of metal that in his apparatus at present constructing and distributed into 50 pairs, will burn 108 feet of steel wire  $\frac{7}{8}$  of an inch thick, if concentrated into one, will have the same effect on 5400 feet of the same wire; an enormous increase of a power that may become as noble a present to the arts, from science, as the steam-engine has proved.

I have also turned my thoughts towards making a new form of apparatus, on the above principle, so as to condense it into as little bulk as possible. The simple inspection of the drawing accompanying this will sufficiently illustrate it; the scale is of three inches to a foot, (Pl. XV.) it represents part of the ground plan of a trough, of about ten feet long, two feet broad, and two and half feet deep: the black line represents the copper, the dotted the

Increase of galvanic combustion.

by one single plate or pair.

Description of the proposed apparatus.



the zinc, the white the acid liquor, and the shaded part the wood: the scale is made large for the sake of distinctness; two spigots or turncocks are adapted to the end by which the liquor may be drawn off; a contrivance of great use, but impracticable in the present form; only two convolutions of the plates are expressed, and the turns are made about three inches wide, to facilitate the soldering of the zinc plates to the copper; the liquor is confined in as small a space as possible, to prevent the unnecessary waste of acid. It may also be constructed with one metal only, on Mr. Davy's plan; in that case, the sulphuret will occupy one side of the plate, and the acid the other. Hoping that the above may merit a place in your very excellent Journal, and be of use to my fellow labourers in science,

I remain, Sir,

Your most obedient humble Servant,

I. R. I. an old Correspondent.

Edinburgh, March 13, 1804.

## VII.

*A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake and other American Serpents. By BENJAMIN SMITH BARTON, M. D\*. From the American Transactions, Vol. IV.*

FIDEM NON ABSTULIT ERROR.

Naturalists frequently too credulous.

**N**ATURALISTS have not always been philosophers. The slight and superficial manner in which they have examined

\* Since this memoir was read before the Society, it has been considerably altered, and somewhat enlarged. I hope, the alterations will render it more worthy of the notice of those who, like myself, derive pleasure and happiness from the contemplation of the works and operations of nature, on this globe.

I fear, I shall be thought to have treated the question in too diffusive a manner. I have not, indeed, laboured to be concise. But if the memoir is more extensive than was necessary, I flatter myself, it will be admitted that it, at least, contains some new and interesting facts. I submit it to its fate. B. S. B.

This interesting paper has been deferred from the pressure of other matter of more immediate and temporary claim. W. N.

many

many of the subjects of their science; the credulity which has accompanied them in their researches after truth, and the precipitancy with which they have decided upon many questions of importance, are proofs of this assertion.

There is a question in natural history that has, in an especial manner, solicited from me these observations. I mean the question concerning the fascinating faculty, which has been ascribed to different kinds of American serpents. It is my intention to examine this question, in the memoir which I now present to the Philosophical Society.

Fascination by snakes.

Of this fascinating faculty we have all heard and read. In many of our country situations, there is hardly a man or a woman, who will not, when the subject comes to be mentioned, seriously relate some wonderful story, as a convincing proof of the doctrine. Children seem taught to believe it. I think, it is sometimes one of the earliest prejudices imprinted on their tender minds. It is a prejudice which often increases with their years; and even in that happy period of life when the mind is most firm, and the least propense to the belief of extraordinary things, the ways of which we are not capable of scanning, I have known this prejudice so deeply and so powerfully rooted, as to mock the light and sureness of facts, and all the strength of reasoning.

generally believed in America.

It is not my intention, in this memoir, to give an analysis, or complete view, of every thing that has been written on the subject. Nor is it my intention to examine the many stories, related by authors, in support of the fascinating faculty of serpents. For the first task, I have not leisure; and, as to the second, I should think my time ill employed in pointing out the gross absurdities which seem to constitute a necessary part of many of those stories. I think it proper, however, to observe, that I have anxiously sought for, and have patiently perused, the volumes of tales published in favour of the doctrine which I mean to controvert.

Examination of the question proposed.

I aim at giving a general, though correct, view of the question, uninfluenced by the bold assertions of ignorance, or by the plausible conjectures of science. In the investigation of the question, I have sought for facts: these have been my guides. I have studiously endeavoured to follow where they seemed to lead. Perhaps, they have led me astray.

General view.

The

The manner in which the supposed fascinating power of serpents is exerted has often been related, by different writers. I shall endeavour to convey some idea of the business, in as few words as I can.

Description of  
the supposed  
process of snakes  
in fascinating  
other animals.

The snake, whatever its species may be, lying at the bottom of the tree or bush upon which the bird or squirrel sits, fixes its eyes upon the animal which it designs to fascinate or enchant. No sooner is this done than the unhappy animal (I use, for the present, the language of those who differ from me in opinion, on this subject) is unable to make its escape. It now begins to utter a most piteous cry, which is well known by those who hear it, and understand the whole machinery of the business, to be the cry of a creature enchanted. If it is a squirrel, it runs up the tree for a short distance, comes down again, then runs up, and, lastly, comes lower down. "On that occasion," says an honest but rather credulous writer \*, "it has been observed, that the squirrel always goes down more than it goes up. The snake still continues at the root of the tree, with its eyes fixed on the squirrel, with which its attention is so entirely taken up, that a person accidentally approaching, may make a considerable noise, without the snake's so much as turning about. The squirrel as before mentioned comes always lower, and at last leaps down to the snake, whose mouth is already wide open for its reception. The poor little animal then with a piteous cry runs into the snake's jaws, and is swallowed at once, if it be not too big; but if its size will not allow it to be swallowed at once, the snake licks it several times with its tongue, and smoothens it, and by that means makes it fit for swallowing †."

Other accounts  
are not material-  
ly different.

It would be easy to cite, from different authors, other accounts of the manner in which the enchantment is performed; or, more properly speaking, of the conduct, or behaviour, of the enchanting and enchanted animals. But between these accounts, there is hardly a specific difference. There is considerable unity in all the relations that I have heard, or read.

\* Professor Peter Kalm.

† Travels into North-America; containing its natural history, and a circumstantial account of its plantations and agriculture in general, &c. &c. vol. i. p. 317 & 318. Also vol. ii. p. 207, 208, 209 & 210. English Translation. London: 1770 & 1771.

However,



However, those who wish to examine this part of the subject more fully, will, at least, receive some degree of entertainment from the perusal of the many authors who have believed and asserted, that serpents possess a power of fascinating other animals.

That the belief in the existence of this power should have been so general among the uninformed part of a people, ought not to be wondered at. The human mind, unenlightened by science, or by considerable reflection, is a soil rich in the weeds of superstition, and credulity. It is ever prone to believe in the wonderful, even when this belief, as is often the case, brings with it fears, and cares, and misery. The bondage of the mind in superstitious credulity is great and heavy. Neither religion nor virtue can give it its freedom. This it obtains from science. How important, then, even in this point of view, is the enlargement of the mind by science!

Credulity in the vulgar is to be expected;

But it is, surely, a matter of some astonishment, that this belief should have been admitted, in all the fulness of its extravagance, by men of learning, of observation, and of genius: by those who have the book of nature in their hands; that book which will, in some future and some happier age, eradicate many of the prejudices which disfigure, and which mock the dignity of human nature: by classical scholars, grown old in the disbelief of similar fables, heightened and embellished by the charms of poetry; and also by the infidel, who denies the authenticity of scripture-miracles, few of which, even though they were not shown to be truths, are more improbable than the imaginary fact which I am examining.

but it is strange that men of ability have also admitted these facts.

I have sought to discover the original, or source of this belief. I do not find any traces of it among the ancient writers of either Greece or Rome. I think, it is most likely that no such traces can be found. Lucan, had serpents been thought to possess a fascinating faculty in his age, and in the country in which he lived, would, probably, have availed himself of its existence, in his beautiful account of the march of Cato's army through the Libyan-Desert\*; and had such a notion prevailed in the earlier days of Lactetius, would we not find some mention made of it in the poem *De Rerum Natura*, one of the finest and most varied productions of the human mind? Classical scholars may possibly, however, discover the dawn

This notion is not found in the writings of the ancients.

\* Pharfalia, lib. IX.

of this notion in Greek and Roman authors, unread by me. On this subject, I have not pushed my inquiries as far as I wished to have done. It is not unlikely that I may examine the question more curiously, at some future period.

It is probable that in the mythology of Asia and of Africa, we shall discover some traces of this notion, so intimately connected with the superstitious credulity of a people, and even so naturally arising out of an imperfect view of the manners of serpents.

It does not appear to have originated nor to be much, if at all, admitted among the Indians.

If we believe the Reverend Dr. Cotton Mather \*, Mr. Dudley †, and other persons, who had resided in North-America, we are to look for the beginning of this ridiculous notion among our Indians. How far, however, this is really the case may, I think, be doubted. It is certain that, at present, the opinion is by no means universal among the Indians. Several intelligent gentlemen, who are well acquainted with the manners, with the religious opinions, and with the innumerable superstitious prejudices of the Indians, have informed me, that they do not think these people believe in the notion in question. My friend Mr. John Heckewelder, of Bethlehem, writes to me, that he does not recollect to have heard the Indians say that snakes charm birds; though he has frequently heard them speak of the ingenuity of these reptiles in catching birds, squirrels, &c. Mr. William Bartram says, that he never understood that the nations of Indians among whom he travelled had any idea of the fascinating power of snakes ‡. On the other hand, however, a Mohegan-Indian told me that the Indians are of opinion that the rattle-snake can charm, or bewitch, squirrels and birds, and that it does this with its rattle, which it shakes, thereby inviting the animals to descend from the trees, after which they are easily caught. According to this Indian, his countrymen do not think that the snake, in any manner, accomplishes the business with its eyes. A Choktah-Indian assured me that the rattle-snake does charm birds, &c. but he was honest enough to confess that he

\* The Philosophical Transactions, abridged, vol. v. part ii. no. 339. p. 162.

† Ibid: vol. vi. part iii. no. 376. p. 45.

‡ MS. note, communicated to me by this ingenious gentleman.

did not know in what manner it does it. The interpreter, through whom I conversed with this Indian, said that the snake charms by means of its rattle.

The veneration, or regard, which has been paid to the rattle-snake by certain North-American tribes seems, at first sight, to favour the opinion, that these tribes attributed to this hideous reptile some hidden power \*, perhaps that of fascinating animals. Mr. William Bartram informs me, that the southern Indians, with whom he is acquainted, seem to hold the rattle-snake in a degree of veneration †. Mr. Heckewelder says that, to his certain knowledge, this reptile was once held in particular esteem by the Delawares. He was several times prevented, by these Indians, from killing the rattle-snake, being told that it was their grand-father, and, therefore, must not be hurt. At other times, he was told, he must not kill this snake, because the whole race of rattle-snakes would grow angry, and give orders to bite every Indian that might come in their way ‡. But, of late, especially among those Indians who have had connection with the whites, these ridiculous notions have mouldered away, and our Indians, at present, kill their rattling "grandfather" with as little ceremony as the Eskimaux are said to kill their parents in old-age.

Some North-American tribes had a veneration for the rattle-snake ;

It is obvious, from contemplating the manners and the history of nations, that a part of their religions, and a large part of the fabrick of their superstitious notions, have arisen

which seems to have been part of the religious system grounded on fear.

\* Vis abdita. Lucretius:

† MS. note communicated to me.

‡ In my *Historical and Philosophical Inquiry* (not yet published), I have collected many facts which seem incontestibly to prove, that the mythology, or superstitious religion, of the Americans is a fragment of that mythology whose range in Asia, and in Africa, has been so extensive. Possibly, the veneration, or regard, which was paid to different kinds of serpents in America did not originate in this continent, but had its source in Asia, from which portion of the globe (after a long and laborious attention to the subject) I cannot doubt, that almost all the nations of America are derived. It is unnecessary, in this place, to cite instances of the religious veneration which was, and still is, paid to some species of serpents, in various parts of the old-world. These instances must be familiar to every person, who is acquainted with the historians or with the poets of antiquity, and with the history of the Gentoo-Indies.



out of fear. Perhaps, all mankind \* admit the existence of two great beings, the one good and all-benèvolent, the other bad and studious of evil. In our own continent, where, I believe, this notion was universal, certain tribes were assiduous in their adoration of the latter being, whilst the former, whom the light of reason taught them to consider as the source of life, and all their blessings, was merely acknowledged and named, but unworshipped and neglected †. The Delawares, and some other nations who speak dialects of their language, believe that a turtle, of an enormous size, inhabits the deep, and supports upon his back this continent, or, as they call it, island. They say it is in the power of this animal, by diving, to drown the world, as he has already done, in former ages. They, therefore, endeavour to conciliate his friendship and good-will. With this view, they make rattles of the turtle-shell, into which they put small stones, beans, or Indian-corn ‡, and play with this instrument, at their dances. The turtle is greatly esteemed by them; and, in the fulness of a mixed zeal and fear, they even deign to call him *Mannitto*, or God; because, they say, he can live both upon the land and in the water §.

It is not likely that they venerated the snake from a belief of its fascination.

It seems very probable to me, that the veneration for the rattle-snake had its birth in fear, and not in the belief that this reptile possessed the power of fascinating animals. If, as some writers have asserted, the Indians were in possession of absolute specifics for the bite of the rattle-snake, I am of opinion that the veneration for this animal would not have existed; or, at least, that it would not long have continued. But the Indians are often unable to prevent or to cure the effects of the active poison of this serpent, which not unfrequently destroys them ||.

I return

\* I speak of mankind in the aggregate, and not of individuals among them.

† John De Laet, speaking of the Indians of New-York, has the following words: "Cæterum nullus ipsis religionis sensus, nulla Dei veneratio: diabolus quidem colunt sed non tam solempiter neque certis ceremoniis, ut Africani faciunt," &c. *Novus Orbis seu Descriptionis Indæ Occidentalis Libri xviii. lib. iii. cap. xi. p. 75. Lugd. Batav. 1633.*

‡ Maize.

§ MS. by Mr. John Heckewelder, *penes me.*

|| Adair says, he does "not remember to have seen or heard of an

I return to the more immediate path of my subject.

Among the Indians of South-America, I do not find any traces of the notion that serpents can fascinate other animals. Pifo, the author of the *Natural and Medical History of the two Indies*, seems to have been studious to bring together the extraordinary things which have been related of the rattle-snake. But he says not a syllable concerning the fascinating faculty of this reptile \*.

The South Americans do not possess this notion,

But whatever may have been the native country of the notion which I am considering, it would have been well had it been confined to savages. It is a tale which seems nicely adapted to the wit and society of rude and uncultivated nations. Unfortunately, the progress of error and credulity is extremely rapid. Their dominion is extensive. The belief in the fascinating faculty of serpents has spread through almost all the

but it has spread over all America and Europe.

Indian dying by the bite of a snake, when out at war, or hunting; although they are then often bitten by the most dangerous snakes." *The History of the American Indians*, &c. p. 235. London: 1775. It is certain, from the testimony of many persons, that the bite of the rattle-snake has often proved mortal to the Indians, and others, notwithstanding the boasted specifics of these people. Father Cajetan Cattaneo says, many Indians die with the bite of serpents. "But," observes the father, "it is said they commonly escape with life, when they can quickly apply the remedy which providence has prepared of certain herbs, especially the spikenard, which some parts of Paraguay produce in plenty. But when they are bit by the rattle-snake it is confidently assured that the case admits no cure." *The third letter of F. Cajetan Cattaneo*. See *A Relation of the missions of Paraguay*, wrote originally in Italian, by Mr. Muratori. English Translation. p. 260. London: 1759. Father Cattaneo is here speaking of the South-American rattle-snake, the poison of which, I have little doubt, is more deleterious than that of the same animal in our part of North-America. Still, however, I am confident, that this poison, even in the most fervid climates, is not always mortal.

\* Gulielmi Pisonis medici Amstelædamensis de Indiæ utriusque re naturali et medica libri quatuordecim. Amstelædami: apud Elzevirios, 1658. Some of Pifo's assertions concerning the rattle-snake are very extravagant. Such are the following: "Caudæ extremitate in anum hominis immissa, mortem inferit confestim; venenum autem quod ore vel dentibus infundit, multo lentius vitam tollit." p. 275.

civilized

civilized parts of North-America. Nor is it confined to America. It has made its way into Europe, and has there taken possession of the minds of scholars, of naturalists, and of philosophers.

It is said that Dr. Johnson believed it. But, he was credulous, timid, and melancholy.

I think, I have somewhere either heard or read that the tale was credited by the late Dr. Samuel Johnson. If I am mistaken, I hope the admirers of this great man, should any of them read my memoir, will pardon me. It is certain, notwithstanding the vast strength and the rich fertility of Johnson's mind, that he was credulous and timid. Did this union of credulity and timidity arise out of that unhappy melancholy ("those casual eclipses which darken learning",) that often overclouded the brightness of the mind\*? We are told that the Hercules of English literature believed in ghosts, and in the second-sight. The man who would thus suffer his mind to be estranged from probability, and entangled in difficulties, would, perhaps, find it easy to bend to the belief, that serpents have the faculty of fascinating other animals.

Linnæus admits it.

Although I profess myself to be a warm admirer of Linnæus, and although, at a very early period of my life, I enlisted myself under the banner of his school, I shall not, nevertheless, attempt to conceal, that this great man gave credit to the tale of the fascination of birds and other animals by serpents. In his *Systema Naturæ* (that immortal work), under the article *Crotalus horridus*, or the rattle-snake, he has the following words: "*Aves Sciurosque ex arboribus in fauces revocat.*"†. In another work, he speaks as follows. "Whoever is wounded by the Hooded Serpent (*Coluber Naja*) expires in a few minutes; nor can he escape with life who is bitten by the rattle-snake, (*Crotalus horridus*) in any part near a great vein. But the merciful God has distinguished these pests by peculiar

\* Or, did his melancholy grow out of his credulity and fear?

† See volume first, p. 372. Vienna edition of 1767. Professor Gmelin, in his edition of the *Systema Naturæ*, when speaking of the rattle-snake, has the following words, viz. "*aves sciurique ex arboribus non raro in fauces inbiantis apertas incidunt,*" tom. i. pars iii. p. 1080. The same laborious author speaking of our grey-squirrel (*Sciurus cinereus*) says, "a crotalo comeditur," tom. i. p. 147. This is true: but he might have said the same when speaking of the striped-dormouse, or ground-squirrel (*Sciurus striatus*), of our rabbit (*Lepus americanus*), and many other animals.

signs,



signs, and has created them most inveterate enemies; for as he has appointed cats to destroy mice, so has he provided the Ichneumon (*Viverra Ichneumon*) against the former serpent, His account, and the Hog to persecute the latter. He has moreover given the *Crotalus* a very slow motion, and has annexed a kind of rattle to its tale, by the motion of which it gives notice of its approach: but, lest this slowness should be too great a disadvantage to the animal itself, he has favoured it with a certain power of fascinating squirrels from high trees, and birds from the air into its throat, in the same manner as flies are precipitated into the jaws of the lazy toad.”\*

Linnæus was, certainly, extremely credulous, though I do not find that any of his professed biographers have taken notice of this feature of his mind. But the proofs of my observation are numerous: they are to be found in almost every essay that he has written. His credulity with respect to the powers of medicines is, perhaps, peculiarly striking †. How far this credulity, in a mind otherwise truly great (a mind which with respect to the arrangement of natural bodies has never been equalled), is to be sought for in the general character of the country which gave Linnæus birth, I shall not pause to inquire. Yet in an investigation of this kind, where the opinion of the Swedish Pliny is necessarily mentioned, it might be curious to look to the sources of his credulity. The study of nature, as it respects this globe, is, perhaps, of all the sciences, the most unfavourable to superstition, or credulity. But the greatest of naturalists was one of the most credulous of philosophers.

It is certain that Linnæus was credulous.

It is proper, however, to observe, in this place, that Linnæus by no means asserts, that he himself had ever been a

He never was himself a witness to the fact.

\* See Reflections on the Study of Nature, translated from the Latin of Linnæus. p. 33 & 34. Dublin edition, 1786. Dr. I. E. Smith, the ingenious translator of this dissertation, in a note to the above passage, has the following words. “This opinion of the fascinating power of the Toad has been refuted, and the appearance which gave rise to it fully accounted for, by Mr. Pennant, in his British Zoology. Probably the story of the rattle-snake’s having a similar power might be found equally false, if enquired into with the same degree of accuracy.” p. 34.

† See his Materia Medica, liber. i. de Plantis, &c. Amstelædami: 1749.

witness

witness to the fascinating power of any of the serpent-tribe. He seems to have received the tale from some of his many pupils, whom he animated with the love of natural history. It is probable that Kalm, whom Linnæus quotes upon various occasions, and whom he could not but esteem, principally contributed to fix his illustrious master's credulity in this respect: for, in different parts of his *Travels*, this industrious author has given his decided assent to the tale; and he informs us, that he has treated of the same subject, more fully, in a treatise which is printed in the *Memoirs of the Royal Swedish Academy of Sciences*, for the year 1753 \*.

Nor was Kalm ;  
though he sup-  
ports it,

Kalm is candid enough to tell us, that he never saw an instance of the fascinating power of the serpent-kind. " However," says he, " I have a list of more than twenty persons, among which are some of the most creditable people, who have all unanimously, though living far distant from each other, asserted the same thing †." He then goes on to tell us a long story, similar to that which I have related, in the beginning of this memoir, and which, therefore, it is not necessary to repeat, in this place.

and speculates  
upon it.

Our author is not content to make mere mention of the fact; he undertakes to speculate upon it. And here, although a talent for ingenious reasoning is, certainly, not the most striking feature that is displayed in the *Travels* of Kalm, he acquits himself, for some time, very judiciously; but spoils all he has said, by concluding, that the bird or squirrel " are only *enchanted*, whilst the snake has its eyes fixed on them. ‡" He allows that " this looks odd and unaccountable, though," says he, " many of the worthiest and most reputable people have related it, and though it is so universally believed here," that is in New-Jersey, &c. " that to doubt it would be to expose one's self to general laughter §."

Several American writers have adopted the notion, that snakes are endued with a fascinating faculty. Fearful that their authority may extend the empire of this error, I have been the more anxious to offer my sentiments on the subject to the society ||.

It

\* *Travels into North-America*, &c. vol. i. p. 318 & 319.

† *Ibid.* vol. ii. p. 207 & 208.

‡ *Travels into North-America*, &c. vol. ii. p. 210.

§ *Ibid.*

|| Speaking of the rattle-snake, my worthy friend Mr. William Bartram

It has given me pleasure to find, that the enchanting faculty of the rattle-snake is doubted by some very respectable European naturalists. "It is difficult," says my excellent friend Mr. Pennant, "to speak of its fascinating powers: authors \* of credit describe the effects. Birds have been seen to drop into its mouth, squirrels descend from their trees, and leverets run into its jaws. Terror and amazement seem to lay hold on these little animals: they make violent efforts to get away, still keeping their eyes fixed on those of the snake; at length, wearied with their movements, and frightened out of all capacity of knowing the course they ought to take, become at length the prey of the expecting devourer, probably in their last convulsive motion †."

Respectable European naturalists doubt it.

My friend Mr. de la Cépède, one of the most eloquent naturalists of the age, has devoted a good deal of attention to the subject, in his *Histoire Naturelle des Serpens*, a work of extensive and superior merit. I regret, however, that this ingenious author was not in possession of a few facts, well known in this country, which could not have failed to conduct a mind, like his, strengthened by the enlarged contemplation of the objects of nature, to the fulness and certainty of truth. As it is, however, Mr. de la Cépède deserves our thanks for reviving, and giving a new turn to, the speculations of naturalists on this subject.

The subject treated by la Cépède.

Bartram says: "They are supposed to have the power of fascination in an eminent degree, so as to enthral their prey. It is generally believed that they charm birds, rabbits, squirrels, and other animals, and by stedfastly looking at them, possess them with infatuation; be the cause what it may, the miserable creatures undoubtedly strive by every possible means to escape, but alas! their endeavours are in vain, they at last lose the power of resistance, and flutter or move slowly, but reluctantly towards the yawning jaws of their devourers, and creep into their mouths, or lay down and suffer themselves to be taken and swallowed." *Travels through North and South Carolina, Georgia, East and West Florida, &c.* p. 267. Philadelphia: 1791.

\* "Lawson—Catesby—Ph. Tr. abridg. ix. 56, &c. vii. 410.—Brickel's Hist, Carolina, 144.—Beverley Virginia, 260.—Colden, i. 12. Dr. Brickel is an author of no credit. His *History of North-Carolina*, here quoted is one of the most daring and scandalous instances of plagiarism I am acquainted with.

† Arctic Zoology, vol. ii. p. 338. London: 1792.

I beg



I beg leave, in this place, to quote that part of Mr. de la Cépède's work which relates to the question of my memoir.

His account.  
Supposition of  
infection in the  
breath.

Speaking of the boiquira, or rattle-snake, my ingenious friend has the following words: "His infectious breath, which sometimes agitates the little animals he is about to seize, may also prevent their escape. The Indians relate, that a rattle-snake is often seen, curled round a tree, darting terrible glances at a squirrel, which after expressing its fear by its cries and its tremour, falls at the foot of the tree, where it is devoured. Mr. Vofmaer (at the Hague), who has made several experiments on the bite of a rattle-snake, which he had alive, says that the birds and mice, which were thrown into the cage, would immediately endeavour to squat in a corner, and that soon after, as if seized with deadly anguish, they would run towards their enemy, who continually shook his rattles: but this effect of a mephitick and fetid breath has been so much exaggerated, and misrepresented, that it becomes miraculous.

Fascinating property  
supposed to  
arise from this  
cause.

"It has been said," continues our author, "that the rattle-snake had a faculty of enchanting, as it were, the animal he intended to devour; that by the power of his glance, he could oblige the victims to approach by small degrees, and finally to fall into his mouth; that even man could not resist the magic force of his sparkling eyes; and that under violent agitations he would expose himself to the envenomed tooth of the serpent, instead of endeavouring to escape. If the rattle-snake had been more generally known, and if his natural history had engaged more attention, other circumstances, still more extraordinary, would have been added to these miraculous feats; and how many fables would not have been substituted to the simple effect of a pestilential breath, which, however, has by no means been either so frequent or so fatal as some naturalists have believed.

Or that the  
animal may have  
been already  
bitten.

"We may presume, with Kalm, that, for the most part, when a bird, a squirrel, or any other animal, has been seen precipitating itself from the top of a tree into the jaws of a rattle-snake, it had been already bitten \*; that after escaping,  
it

\* I do not find that Kalm has adopted this system of explanation, in his *Travels*. On the contrary, in this work, he gives some judicious reasons for rejecting this mode of explanation. *Travels*, &c.

manifested, by its cries and its agitation, the violent action of the poison left in its blood, and diffused through its circulation, by the envenomed inoculation of the reptile's tooth; that, its strength gradually decaying, it would fly or leap from branch to branch, till finally exhausted it would fall before the serpent, who with inflamed eyes, and eager looks, would watch attentively every motion, and then dart on his prey, when it retained but a small portion of life. Several observations related by travellers, and particularly a fact mentioned by Kalm, appeared to confirm this\*."

From this long quotation, it appears that Mr. de la Cépède adopts two modes, or circumstances, for explaining the miraculous power, which has been attributed to these serpents. The explanation is, undoubtedly, in both cases, ingenious, and entitled to notice. I shall examine the question with that attention which it deserves.

Examination of these doctrines.

In the first place, my learned friend supposes, that the rattle-snake's infectious breath †, by agitating the little animals which it means to devour, may prevent their escape.

I do not altogether understand this expression of an infectious breath. I do not think that we are in possession of any facts by which it can be proved, that the breath of the rattle-snake is, in general, more infectious, or pestiferous, than that of many other animals, whether of the same or of a different family. I know, indeed, that in some of the larger species

Observation to show that snakes do not emit any offensive odour,—

&c. vol. ii. p. 209 & 210. His memoir, in the *Swedish Transactions*, I have not seen. Sir Hans Sloane, a long time since, conjectured, that the whole mystery of the fascinating faculty of the rattle-snake is this, viz. "that when such animals as are the proper prey of these snakes, as small quadrupeds, birds, &c. are surprised by them, they bite them, and the poison allows them time to run a small way; or perhaps a bird to fly up into the next tree, where the snakes watch them, with great earnestness, till they fall down, or are perfectly dead, when having licked them over with their spawl or spittle, they swallow them down." *Philosophical Transactions*, vol. xxxviii. no. 403. M. de la Cépède does not appear to have seen Sloane's paper:

\* *Histoire Naturelle des Serpens*, p. 409, 410 & 411. a Paris: 1789.

† His words are, "son haleine empestée, qui trouble quelque-fois les petits animaux dont il veut se saisir, peut aussi empêcher qu'ils ne lui échappent." p. 409.

of

of serpents, inhabiting South-America, and other countries, there is evolved in the stomach, during the long and tedious process of digestion in these animals, a vapour, or a gas, whose odour is intensely fetid. I have not, however, found that this is the case with the rattle-snake, and other North-American serpents, that I have examined. But my own observations on this head have not been very minute. I have made inquiry of some persons (whose prejudices against the serpent-tribe are not so powerful as my own), who are not afraid to put the heads and necks of the black-snake, and other serpents that are destitute of venomous fangs, into their mouths, and have been informed, that they never perceived any disagreeable smell to proceed from the breath of these animals. I have been present at the opening of a box which contained a number of living serpents; and although the box had been so close as to admit but a very small quantity of fresh air, although the observation was made in a small warm room, I did not perceive any peculiarly disagreeable effluvia to arise from the bodies of these animals. I am, moreover, informed by a member of this society\*, who, has, for a considerable time, had a rattle-snake under his immediate care, that he has not observed that any disagreeable vapour proceeds from this reptile. On the other hand, however, it is asserted by some creditable persons of my acquaintance, that a most offensive odour, similar to that of flesh, in the last stage of putrefaction, is continually emanating from every part of the rattle-snake, and some other species of serpents. This odour extends, under certain circumstances, to a considerable distance from the body of the animal. Mr. William Bartram assures me, that he has observed "horses to be sensible of, and greatly agitated by, it at the distance of forty or fifty yards from the snake. They showed," he says, "their abhorrence, by snorting, winnowing, and starting from the road, endeavouring to throw their riders, in order to make their escape†." This fact related by a man of rigid veracity, is extremely curious; and, in an especial manner, deserves the attention of those writers, who, like Mr. de la Cépède, imagine that this fetid emanation from serpents is capable of affecting birds,

and others to  
show that they  
do.

\* Mr. Charles Wilson Peale.

† MS. note, communicated to me.



at small distances, with a kind of asphyxy \*. It even gives some colour of probability to the story related by Metrodorus, and preserved in the *Natural History* of Pliny †.

(To be continued.)

### VIII.

*Extract of a Letter from Brugnatelli on the Prussiate of Potash, and on the Preparation of a fulminating Muriate of Silver.\**

IF alcohol be poured into a solution of prussiate of potash (oxiprussiate of potash,) the liquor becomes turbid, and a precipitate is deposited in the form of small and very brilliant lamellæ, resembling the sublimed boracic acid (oxiboracic.) Collecting this precipitate on a filter, and drying it, the scales adhere to one another, and form a stratum of a shining yellowish white, like that of mother-of-pearl. This mass easily separates from the filter; is soluble in sulphuric acid (oxisulphuric), and this solution takes a blue colour on the addition of water. Exposed to the fire in a crucible, it swells, burns, and leaves a coally residuum, containing a great deal of iron, attractable by the magnet and potash. This coally residuum, treated with acids, gave out a strong smell of sulphurated hydrogen gas (*gas phlogogène sulfuré.*)

Alcohol occasions a precipitate in solution of prussiate of potash, which is small scales, adhering to each other, of a shining, yellowish white, soluble in sulphuric acid; and this solution is turned blue by water. Its coally residuum contains a great deal of magnetic iron, and potash; and with acids smells strongly of sulphurated hydrogen gas.

I have applied the process I mentioned to you in my last to a great number of other metallic oxides (thermoxides), in hope of rendering them fulminating; but I succeeded with none, except that of silver.

Take a hundred grains of lunar caustic (fused oxiseptonate of silver) in powder, put them into a beer glass, and pour on them first an ounce of alcohol, and then as much concentrated nitrous acid (*oxiseptoneaux.*) The mixture grows hot, enters into a state of ebullition, and an ether is visibly formed, that changes into a gas. By degrees the liquor becomes milky and opaque, and is filled with small and very white clouds. When all the gray powder of the lunar caustic has taken this form, and the liquor has acquired a consistency, distilled water must

On 100 grains of nitrate of silver powdered was formed first, alcohol 1 ounce, then nitrous acid 1 ounce.

As soon as the precipitate was completely formed, water was poured on, and the precipitate dried in a filter.

\* *Histoire Naturelle des Serpens*, p. 355.

† *Lib.* xxviii. cap. 14.

‡ *Van Mons's Journal*, IV. 235.

be added immediately, to suspend the ebullition, and prevent the matter from being redissolved, and becoming a mere solution of silver. The white precipitate is then to be collected on a paper filter, and dried.

Its weight more than 50 grains. It detonates more strongly than fulminating mercury, being touched with a glass tube wetted with sulphuric acid,

placed on a lighted coal, or having a spark drawn through it from the electric pile.

This precipitate is fulminating silver, and amounts to more than half the weight of the lunar caustic employed. The detonating power of this preparation greatly exceeds that of fulminating mercury, prepared in Howard's or my way, even in much smaller quantity. It detonates in a tremendous manner, on being scarcely touched with a tube of glass, the extremity of which has been dipped in concentrated sulphuric acid (*oxisulphurique*), even in that of the shops. One grain of this fulminating silver, placed on a lighted coal, gave such a loud report, that it deafened the ears of the persons present. The same thing happens on placing a little of the same preparation on an electric pile, with the interposition of a bit of paper, and drawing a spark from its centre by means of a slip of metal. The paper will be either pierced or torn.

## IX.

*Of the Application of Platina or Porcelain.* By KLAPROTH.†

Platina never yet applied on porcelain.

PLATINA, as far as I know, has never yet been employed in encaustic painting. Accordingly I thought it not amiss, to make some experiments on the subject, the result of which did not deceive my expectations.

Gold and silver only have been used in this way in the metallic state: and gold answers completely, but silver does not cover the ground so well, and is liable to tarnish.

Gold and silver are the only metals, that have hitherto been employed in the metallic form in painting or ornamenting porcelain, glass, or enamel. Gold answers this purpose so completely, that it leaves nothing to be desired: but silver is far from affording equal satisfaction. As it is of less density than gold, it cannot be used so thin, and does not cover the porcelain or other substance so well. Another reason why it is less applicable to the purpose of painting on porcelain is its property of having its metallic lustre tarnished by sulphurous emanations, all kinds of which blacken it. This unfavourable circumstance prevents silver from being employed in fine enamel painting, and confines the use of the metallic substances in that only to gold.

\* Abridged from Scherer's *Journal der Chimie*, 1802. No. 4. p. 413.

Platina

Platina in its qualities ranks with gold, and by its whiteness supplies the place of silver, without labouring under its defects. It not only covers the ground well by its density, which even surpasses that of gold; but like this it resists all the alterations of the atmosphere, and is not any way tarnished by sulphurous effluvia.

Platina imitates the whiteness of silver, without having its defects.

The process for applying it is very simple. Platina is dissolved in nitro-muriatic acid, and precipitated by a solution of muriate of ammonia. The red crystalline precipitate formed is to be dried, reduced to a fine powder, and made slightly red hot in a glass retort. The muriate of ammonia, which had precipitated in combination with the platina, sublimes; and the metal remains at the bottom of the retort, in the form of a light, gray powder. This powder being mixed with a small proportion of flux, as is done with gold, and ground with oil of spike, is to be applied to the porcelain, put into the furnace, and afterwards burnished.

Process of applying it.

Platina applied on porcelain in this manner, is of a silver white, slightly tending to the gray of steel. By alloying this metal in different proportions with gold, different shades of this colour are obtained. Platina admits a considerable quantity of gold, before its colour undergoes any perceptible change to yellow. For example, if one part of platina be alloyed with four parts of gold, the presence of the latter cannot be perceived, and the colour scarcely differs from that of pure platina. The colour of the gold does not predominate, unless it be in the proportion of eight to one.

It resembles silver with a slight steel tinge. Gold in different proportions varies its shade; but does not render it perceptibly yellow, unless 4 parts of gold be added to 1 of platina; and the gold colour does not predominate, unless it be in the proportion of 8 to 1. Platina and silver give a dull alloy. Platina may likewise be applied in solution; and this, if several coats be laid on, penetrates the substance, and produces a mirror resembling polished steel.

The alloys of platina with silver give only a dull metal.

Beside this method of applying platina to porcelain, it may be laid on in the state of solution. In this way its colour, lustre, and appearance, are very different. If the nitro-muriatic solution of platina be evaporated to a certain consistence, and laid on the porcelain several times, the metal penetrates into its substance, which, after it comes out of the furnace, exhibits a metallic mirror of the colour and brilliancy of polished steel.



## X.

*Abstract of a Memoir on Galvanism, sent to the National Institute by Mr. RITTER, of Jena.†*

Erman discovered, that an imperfect conductor would take the electricity from an insulated pile, and become charged like it, one end +, the other —.

TO form a just idea of these researches, it is necessary to recal to mind a discovery made by Mr. Erman of Berlin, about two years ago, and since repeated by Volta before the galvanic committee of the Institute.

If an electric pile, the superior pole of which is positive, and the inferior negative, be insulated, and a communication is made between these two poles, by means of an imperfect conductor, as for instance, a slip of paper wetted with pure water would be for such small quantities of electricity; each moiety of this slip would take the electricity of the pole with which it communicated; the upper part would be positive, the lower negative.

If this be removed by a glass rod, its equilibrium will not be restored at once;

Now let us conceive this imperfect conductor to be removed by an insulating substance, as a glass rod: the equilibrium between the two extremities will not be restored instantaneously, but they will remain positive and negative for some time, as when they communicated with the two poles of the pile.

but gradually.

This difference will gradually diminish in proportion as the opposite electricities recombine, and their action, being neutralized, will soon become altogether imperceptible.

For this Mr. Ritter substitutes a pile of copper and wet pasteboard in alternate pieces.

The fundamental experiment of Mr. Ritter is precisely reducible to this; he merely substitutes for the slip of paper, a pile composed of disks of copper and wet pasteboard. This pile, incapable itself of setting electricity in motion, at least if we suppose all its component parts homogeneous, is charged by communication with the pile, like the slip of wet paper above-mentioned.

Weak electricity, like light, does not pass freely from one surface to another. Hence Mr. Ritter's pile remains charged for some time;

But there is an essential difference in the results. It appears that electricity, when weak, experiences, like light, a kind of difficulty in passing from one surface to another: at least this seems to follow from the experiments of Mr. Ritter, as he himself observes. The electricity introduced into the pile made with one metal alone, experiences therefore some resistance in passing from the metal to the pasteboard; and this obstacle in-

\* Bulletin des Sciences, No, 79, p. 145. October, 1803.

creases, in proportion as the alternations are more numerous. Accordingly, this pile, when once charged, must lose its electricity very slowly, if there be no communication between its poles.

But if a communication between its two poles be established, by means of a good conductor, the current of the two electricities and their combination, taking place swiftly, will produce a discharge, which will act, as in the Leyden phial, by an instantaneous shock. To this effect a new state of equilibrium will succeed, in which the intensities of the different plates will be diminished in proportion to the quantity of electricity that is instantaneously neutralized. These discharges must therefore be reiterated; but growing feebler each time, as the contacts are repeated: but they will soon cease to be perceptible, in consequence of the general equilibrium they tend to restore between all the parts of the apparatus; in a word, the action of this pile depends on its becoming alternately a better and worse conductor, according as the two extremities are made to communicate with each other or not.

As to the mode in which the electricity disposes itself, it must be such, that the repulsive power, or intensity of each plate, combined with the resistance of the surfaces, must counterbalance the united actions of all the rest. In consequence, if the plates of metal be an uneven number, and the whole apparatus insulated, the intensities will diminish uniformly from the two extremities, where they will be equal and contrary, as in the primitive pile, to the centre, where they will be null: but if the base of the pile have a communication with the ground, the intensities will increase throughout the whole of the pile, from the bottom, where they will be null, to the top, where they will equal that of the primitive pile.

The apparatus just described, which Mr. Ritter calls a secondary pile, produces shocks, the decomposition of water, and all the other physiological or chemical effects obtained from the common pile, but with less intensity. By varying the number and order of the disks of pasteboard and copper, Mr. Ritter obtained several interesting results. Thus he observed, that of all the modes, in which a certain number of heterogeneous conductors could be disposed, the arrangement that has least alternation is most favourable to the propagation of electricity. For instance, if a pile be constructed of sixty-four

unless a communication be made between its poles, when it will give a shock, like the Leyden phial.

Thus the intensities of the plates will be diminished; till by repeated discharges, each weaker and weaker, the equilibrium is restored.

Its action explained.

The electricity is greatest at the extremities, nothing in the centre, if insulated; or greatest at the top, and nothing at the bottom, if it communicates with the ground.

This secondary pile produces all the effects of the common pile, but weaker.

The fewer the alternations, the more speedily the electricity flows off,



and the contrary.

Though weak electricity experiences resistance in passing from one surface to another; electricity strong enough to overcome this passes off at once.

As the arrangement affected the conducting power, it might be supposed it would influence the effects.

And it appeared, that the fewer the alternations, the greater the chemical effect;

and on the contrary, the more numerous the alternations, the greater the physiological effect.

The former therefore is produced by a brisk continued flow of the electricity; the latter by repeated shocks.

plates of copper, and sixty-four pieces of wet pasteboard, disposed in three parcels, all the pasteboards being placed together in the middle, and thirty-two plates of the metal at each end, this pile will conduct the electricity from Volta's very freely, and consequently charge itself very little. If the series of wet conductors be interrupted by a single plate of copper placed in the midst of them, the conducting power will diminish; more frequent interruptions will enfeeble it still more; and thus by increasing the number of interruptions we arrive at a series, in which the conducting power will be scarcely perceptible. These phenomena led Mr. Ritter to the knowledge of the resistance, which a weak electricity experiences in passing from one surface to another: and this resistance takes place only in a weak state, for by a singular property, an electricity strong enough to overcome it opens itself a free passage, and flows off completely.

We have just seen, that the conducting power of an apparatus might be varied at pleasure, by altering its arrangement. It was natural to suppose, that these modifications would influence the chemical and physiological effects of the secondary pile; and Mr. Ritter proposed to himself, to ascertain the differences of this influence.

Accordingly he sought how to divide a given number of wet and solid conductors so as to form a secondary pile, that should receive the greatest possible charge from a given electrical pile. Pursuing this inquiry, he discovered two different arrangements, one of which gave a maximum of chemical effect, the other of physiological. The first consists in a small number of alternations. In this case the conducting power is very great, the flow of the fluid continual, and the physiological effect but weak. The second, on the contrary, consists in more numerous alternations; in which the conducting power is much less, and takes place only at intervals, in momentary discharges, when the resistance of the surfaces is overcome. In this the electricity escapes as it were by jerks, and the chemical effect resulting from it is scarcely perceptible. These differences appear to us to indicate, that the chemical effects depend particularly on a brisk continuous current of the fluid, while the physiological require successive sudden discharges, entering the organs as if by shocks.

From



From these principles we see, why the apparatus of Mr. Ritter's pile is better adapted to separate these two kinds of action well adapted to separate the two actions. In the common pile the intensity increases with the number of pairs of metal, and balances the resistance arising from these alternations; while in the secondary pile the intensity of the two extremities can never surpass that of the primitive pile, and the resistance produced by the alternations is entirely employed in modifying the current of a given quantity of electricity.

In fine, if Volta's pile can thus change the secondary pile of Ritter, this property is owing to its intensity being extremely weak, and as we may say imperceptible. A more powerful electricity, such for example as that of the common electrical machine, would pass through the apparatus completely and not produce the same effects. But Volta's pile could not charge Ritter's, if its electricity were not very weak.

Though these deductions appear to us very natural, we offer them only with extreme reserve, and because we think they agree very well with the facts observed by Mr. Ritter. These deductions not advanced as certainties.

The differences that exist in the chemical action of common piles, in proportion to the size of the plates, occur likewise in the secondary piles. The arrangement of the pieces of paste-board, their thickness, the nature of the solution with which they are wetted, and the order in which they are interposed, as well as various other trifling circumstances, modify the effects in a thousand ways, which it would be both useful and curious to investigate. Causes of difference in the secondary pile. It would be useful and curious to investigate its effects.

## XI.

*On Spectacles; particularly the Periscopic. In a Letter from Mr. E. WALKER.*

To Mr. NICHOLSON.

*Lynn, March 19, 1804.*

SIR,

ALTHOUGH the improvements which have been made in optics within the last century, have been such as even to exceed the expectations of a Newton, yet our spectacles, the most useful of all optical instruments, have remained for ages in the same state of imperfection. Many alterations, it is true, Spectacles have not been improved as much as other optical instruments.

have taken place in their form, but without producing any better optical effect; and this may occasion a suspicion, that the properties of the glasses made use of, have not been clearly understood.

General position  
that the rays  
ought to fall  
perpendicularly  
on the lens.

Emerson observes, that "when glasses are put into frames for spectacles, these frames ought not to be straight, so as both glasses may lie in the same plane; but they ought to be so bent in the middle, that the axis of both glasses may be directed to one point, at such a distance as you generally look with spectacles. By this means the rays will fall perpendicularly upon both glasses, and make the object appear distinct. But if they fall obliquely upon the glasses, it will cause a confused appearance in the object \*." Martin also bent the frames of his visual glasses, for the same purpose as above described. And it appears from Dr. Wollaston's description of his *Periscopic Spectacles*, that they are also constructed on a principle to avoid oblique rays †.

Affertion:  
that the perpen-  
dicular rays are  
least adapted  
to distinct  
vision.

It is very singular, that this mode of constructing spectacles should have been recommended by so many writers on optics, when it is easily proved, that the rays of light which fall perpendicularly upon the centre of a lens, enter the eye placed near it, in a more confused state than those rays which fall upon a lens obliquely.

General obs.  
of Dr. Smith.

Dr. Smith, in his complete system of optics, gives us this general observation on vision. "That the apparent distinctness and confusion of an object depends upon the mutual inclination of the rays to each other in any one pencil when they fall upon the eye." ‡

Illustration;  
by which it is  
maintained that  
a pencil of rays  
passing near the  
axis of a lens  
will afford vision  
less distinct

This observation being admitted, let *LL*, *Fig. 1.* represent a double convex lens, *ao*, a ray of light passing perpendicularly through its centre; *cn*, and *dn*, two other rays nearly parallel to the former. The ray *ao* suffers no refraction; but the ray *cn* by passing through the lens, will be refracted towards the right hand, and the ray *dn*, by refraction, will be turned towards the left; consequently these rays have not a mutual inclination to each other when they fall upon the eye placed near the lens, as at *x*.

\* Emerson's Optics, page 167.

† Philosophical Journal, V. VII, p. 143.

‡ Elementary parts of Smith's Optics, page 76.

Fig. 1.

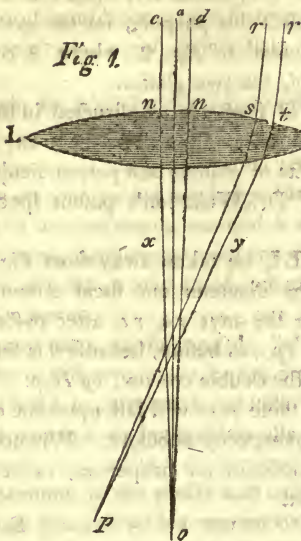
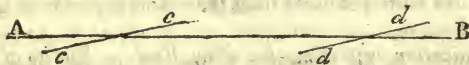


Fig. 2.



But, let  $rs$ ,  $rt$ , represent two other rays falling upon the lens in an oblique direction; these rays after passing through the lens, will both be refracted towards the left hand, and will therefore have “a mutual inclination” to each other when they fall upon the eye at  $y$ , turned to view an object in the direction  $ry$ ; hence an object viewed in that direction, will be seen more distinctly, than if it were viewed through the centre of the lens.

The same thing may be proved experimentally, thus; let a person when reading with spectacles that are proper for his sight, look attentively for a moment, at a single word through the centres of his glasses. Then let him turn his head, either to the right or to the left, (without moving the book) so that the same word may be seen by oblique rays, and it will appear much more distinctly than before.

From what has been advanced it appears, that when glasses are put into frames for spectacles, those glasses should stand in an oblique direction, that the rays of light issuing from an object,

—than a pencil passing farther from the center.

Experiment offered in support of this doctrine.

Inference: That spectacle glasses ought to be fixed obliquely,



ject, may not fall perpendicularly upon them. Bending the frames will not effect this purpose, for the glasses must be parallel, as represented in *Fig. 2.* where *AB* represents the frame, *cc* and *dd*, the two glasses.

I have a pair of spectacles constructed in this manner, but so contrived, that the glasses may be set to any angle, to show the different effects of oblique and perpendicular rays.

The theory of Dr. Wollaston's patent spectacles, may be thus examined.

and that the meniscus is not superior to the double convex.

Let the lens *LL* be taken away from *Fig. 1.* and a meniscus of the same diameter and focal distance be drawn in its place. Then the rays *rs*, *rt*, after passing through this lens will meet at (*p*) as before, because this lens is of the same focal distance as the double convex, *by Hyp.*

Consequently these rays will fall upon the eye at *y*, in the same state of convergency as before. Whence a lens of the meniscus form, possesses no properties, *in theory*, superior to the double convex.

E. WALKER.

## XII.

*Communication respecting the Chay Root, a Species of Madder used for dying durable red Colours in the East Indies. By JOHN STOCKWELL, Esq.\**

Introductory letter to Mr. Taylor.

MR. STOCKWELL presents his compliments to Mr. Taylor, with a sample of an article called Chaya Root, for the inspection of the Society for the Encouragement of Arts, &c. It is in much use on the Coromandel coast, for dying. He accompanies it with a short extract, which will afford some information concerning it.

If Mr. Taylor should cause any experiments to be made upon it, Mr. Stockwell will be obliged by being favoured with the result; and if any person should think favourably of it, and be desirous of a quantity, no doubt it may be had.

*East-India Warehouses,  
Crutched Friars, 23d May, 1803.*

\* Made to the Society of Arts, Adelphi, and printed in Volume XXI. of their Transactions.

*Extract from the Bengal Commercial Consultations, the 24th of April, 1798.*

To W. A. EDMONSTONE, Esq. Secretary to the  
Board of Trade.

SIR,

I HAVE the honour to acknowledge the receipt of your letter of the 31st ult. informing me that I was desired by the Board to make some experiments with the Chay Root, and to report whether I think it will be of any use to the dyers or calico-printers at home.

Letter to the  
Bengal Board of  
Trade.

It is rather surprising, considering the pains that have been taken in Europe to discover, or at least to imitate the method of dying the *Turkish* or *Adrianople* red, that so little attention has been paid to the equally beautiful and permanent red given to their calicoes by the natives of the coast of Coromandel. Although full accounts of the practice of calico-printing in the East Indies were sent home long ago by the missionary Cœur Doux, M. Poivre, and others, it does not appear that the European artists have ever tried their skill in the *Chay* root, the drug by which the admired red colour is produced. I have never heard, at least, of any such attempts, nor do I believe that the root has ever been sent home. It is evident, from the manner in which this drug is mentioned by Dr. Bancroft, in his "Experimental Researches," p. 174, that well-informed writer had never seen it, which I think could scarcely have happened, had it been at all known to the London dyers.

The Chay-root  
affords a beautiful and permanent red to cottons.

Not practically  
known in Europe.

It is probable that the tediousness of the Indian process, as described by those who sent home the accounts of it, consisting of many tiresome manipulations, continued during a period of nineteen or twenty days, deterred the European artists from trying the effects of the *Chay* root in dying or printing their cottons. What may appear more extraordinary is, that the same cause, co-operating perhaps with the natural indolence of the people, and their having cheaper though inferior red dyes at hand, has prevented the use of this root from obtaining in Bengal; for, so far as I can learn, it is not used in this part of India. The natives here are content with the red produced by the *Aul Munjiet* and other drugs, though the colours yielded by these are far inferior to those of Madras calicoes.

The Indian process apparently tedious.

Cheaper drugs  
used at Bengal.

On

On this account I am surpris'd at the measure which has been adopted, of sending round hither such a large quantity of the *Chay root* from the coast. I apprehend very little or none of it will be purchased at the approaching sale, in which case it must be either returned to Madras or sent to England. The expediency of this last measure must depend on the possibility of so abridging the process of dying, or printing cottons with *Chay root*, that the English artists may find it worth their while to have recourse to it for the reds and purples, instead of the *Smyrna madder*, or whatever other drugs they use for those purposes at present.

Experiments to  
shew whether  
the Chay root  
might be of va-  
lue in Europe.

To ascertain this point, I have, agreeably to the instructions of the Board, made several experiments with the *Chay root*; indeed as many as the shortness of the time, and my other avocations would permit. From the result of these trials, I entertain great hopes that the English calico-printers will not only shorten the process, so much as to finish it within a tenth part of the time required for the Indian process, but that they will by the *Chay root* dye their cottons of a brighter red than can be done by madder, or any other vegetable.

Cotton was  
buried or galled;

It is needless to detail the many trials I made, which either failed altogether, or succeeded in a very imperfect degree. I shall therefore only mention that process which I found to answer best, and by which the piece of cotton cloth, which I herewith send you for the inspection of the Board, was printed. Having made a decoction of two ounces of powdered *Hurr*, (the fruit of the *Myrobolona Citrona*\*) in a quart of water, I took a piece of Madras cotton cloth, and boiled it in the decoction for about half an hour. Having taken out the cloth, and washed it well with cold water, I dried it in the sun, and afterwards had it properly ironed and smoothed for the pencil.

then washed;

and a design  
made with ace-  
tite of alumine;

washed,

I then took some of the acetite of alumine, made in the manner directed by Dr. Bancroft, and, thickening it properly with gum-arabic, I delineated a flower with this mordant upon the cloth, and dried it in the sun. I afterwards washed the cloth in cold water, to clear it of the superfluous acetite, and dried it again in the sun. I then infused about two ounces of the *Chay root*, coarsely powdered, with about a quart of water, in a vessel well tinned, and setting it on the fire, as soon as the

\* Aleppo galls will probably answer as well as the *Hurr*.



liquor began to grow warm, I put the cloth into it, and let it remain until it had boiled about half an hour, during which the cloth was frequently stirred. I then took it out, and having rinsed it well with cold water, I put it to dry in the sun. The delineation of the flower now appeared of a good bright red, and the ground of the cloth, though slightly tinged, was much less so than I expected. By washing it again with cold water, and afterwards with soap and water, and exposing it for a whole day to the sun, during which it was frequently besprinkled with water, I brought it to the state in which you now see it. The whole process has only taken up about six and thirty hours. I should have mentioned that the washing with soap heightened the brightness of the red considerably.

and boiled in the infusion of Chay-root.

It gave a good red;

which stood washing, was improved by soap, and cleared by exposure to the sun.

Making allowance for my want of experience in the practice of dying, and considering the great improvement which may be expected in the process from the superior skill of the English artists, I think we may conclude, even from this imperfect essay, that the *Chay root* will be a valuable acquisition to the English calico-printers. I therefore recommend, that so much of the root as may remain unpurchased at the sale, or at least that a part of it may be properly packed up and sent to England by one of the ships now under dispatch.

This rough essay is encouraging.

This drug is the root of a plant called by the botanists *Oldenlandia Umbellata*. I have not met with it in this part of India; but it grows naturally on the coast of Coromandel, where it is also cultivated in great abundance, for the use of the dyers and calico-printers.

Some account of the drug.

The sample which you sent me appears to be of a good quality, and in good condition. It is said that the root will remain with its virtues entire for several years, and that they are even improved by keeping. If this be the case, and provided the dyers at home find it answer, this circumstance is a very favourable one, and must enhance the value of the drug as an article of commerce.

I am, &c.

(Signed)

J. FLEMING,

Inspector of Drugs.

Export Warehouse,  
6th April, 1798.

*Minute of the Board of Trade, dated April 10, 1798.*

Opinion of the Board of Trade, that the root will be acceptable to calico-printers, and of value to the E. I. Company,

THE Board are of opinion, that the result of the Inspector's experiments will afford very acceptable information to the calico-printers in England; and, if their operations should prove his ideas to be well founded, the plant may prove a valuable acquisition to the manufacturers of Great Britain, and also an article of commerce from the coast particularly useful, as there is a want of coast articles of low value, as well light, as a sufficiency of ponderous, to make up a proper cargo for a large ship, without swelling its value to too great a risk, as would be the case were a ship to be loaded entirely with piece goods.

### XIII.

*Description of an Apparatus for raising Water by the Fall of Waste Water. By Mr. SHARPLES, Portrait Painter, Bath. Communicated by the Author.*

Advantages to be derived from the fall of water.

Small mills in Portugal.

A very small brook may do work,

EVERY drop of water that falls may be considered as a natural power capable of doing some work; and it would be productive of considerable benefit in many domestic and manufacturing exigencies, if small streams and currents descending along the sides of hills, or even through the meadows, were employed, by the application of simple machinery, to assist the labours of man. I am informed that numerous little mills for grinding corn are distributed over the face of the country, in Portugal, upon very inconsiderable rivulets; where they not only add to the general product of human work, but may also perhaps tend to equalize the price of grain to the consumer. And indeed if it be considered that a small stream of about a yard wide, running over an obstacle to the depth of an inch, and having space to descend through twelve inches, would do half as much work in the 24 hours as a horse, whether in grinding corn or turning an engine, or raising water for useful purposes, we shall be struck with the necessity of enquiring whether any falling water whatever should be suffered to descend without due employment in its course.

The

The ingenious gentleman who has favoured me with the sketch from which Plate XVI. was made, has directed his invention to teach us how the waste water of our apartments, and other similar places, may be made to raise other water in its descent; and the simple engine he proposes for that purpose consists of the following parts:

A, B, and C represent three vessels or compartments. The upper compartment A is an open trough, or waste receptacle, which can be emptied into the second close vessel B, by turning the cock D; which cock, by the same motion, opens the communication-pipe I and an air-pipe H, to suffer the included air to issue out. N and M are the two legs of a syphon; the shorter N, communicating with the reservoir O, containing water, and the longer M, communicating with a trough P, at a lower level which overflows into the drain. The cock F, by the same turn, opens or shuts both these pipes at once. And lastly, the pipe G forms a communication from the upper part of the receptacle B to the upper part of C. The process may be thus explained:

When D is opened, the middle vessel B receives waste water from A. If D be then shut and F opened, the longer syphon-leg M admits the waste water to flow into P; leaving a space in B wherein the air in C as well as B expands (with a diminished spring) by means of the pipe G. The pressure of the atmosphere on the water in O will therefore raise that fluid into C, provided the height be not too great; and if the difference between the heights of the vessels B and C be equal to that between O and P, the quantity of fresh water raised may equal that of the waste which descends from the greater height.

The cock E serves to draw off the water so raised; at which time it is convenient that the cock F should be shut; and this should also be done whenever the cock D is opened. This may easily be made to take place by a simple connection between the cocks, without depending on the skill or diligence of the servant.

W. N.

Engine for employing waste water.

Description.  
It is a syphon with compartments.

The foul water runs down one leg, and clean water ascends by the other.



## XIV.

*On Gridiron Pendulum Rods composed of Lead and Iron. By Mr. BENZENBERG \*.*

Mr. B. employs lead, on account of its great dilatability. His compensation made by a central rod,  $16\frac{1}{2}$  inches by  $\frac{1}{2}$  an inch. **M**R. B. was induced to employ lead on account of its great dilatability, which is to iron as 2.57 to 1, so that 16.5 inches of lead compensate 1.3 of iron; and he chose iron in preference to steel, because easier to work. The compensation was made by a single rod in the centre,  $16\frac{1}{2}$  inches long, French measure, and  $\frac{1}{2}$  an inch thick. It was simply pinned into gorges in the cross-piece of copper; but the other parts of the gridiron were rivetted in the usual way. The iron rods were made of the best thick iron wire.

Its advantages. The materials of this pendulum are cheap, and it may be made in a couple of days. As the pressure takes place in a vertical direction, there is no danger, according to Mr. B. of rods of these dimensions bending.

Mode of correcting the compensation. To correct the compensation, the central rod of lead must be left so long that we may be sure the compensation is in excess. The quantity of error may then be found by the freezing apparatus, and how much it is requisite to cut from the rod may be calculated with the greatest exactness.

## XV.

*On the Advantages of a large Knife in making Pens. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

The edge of a large knife is better than that of a small one.

**U**SEFUL discoveries are sometimes derived from the common occurrences of life; and that which I am now going to mention, is a mere accident of this kind. About three years ago, I took up a large pocket knife which had been newly ground, and used it for making a pen, and was not a little surprized to find that it answered the purpose much better than

\* *Voigt's Magazin fuer den neuesten Zustande de Naturkunde*, Vol. IV. p. 787.

any penknife I ever had. I mentioned this circumstance soon after to the cutler who made the knife. He said that he had no doubt of the truth of my observation, as he could harden and temper a large blade better than a small one; because there is more difficulty in ascertaining the proper degree of heat in the latter than in the former.

And it may perhaps be a mechanical truth, that a strong knife will overcome the resistance of a hard quill with less exertion of the hand than a weak one. The blade of the knife that I use, and which gave rise to these observations, is four inches in length and half an inch in breadth.

And perhaps the mechanical effect.

It may perhaps be supposed that a knife of these dimensions is improperly constructed for cutting the sides of a pen into a curve, but it will be found, on trial, that the breadth of the blade is no impediment, for it is the form of its edge which it receives by whetting, that renders a broad blade quite as manageable as a narrow one.

It may with convenience and advantage be used as a penknife.

I am, SIR,

A constant Reader of your  
Truly valuable Journal,

March 22, 1804.

W.

## XVI.

*Communication respecting an Article sent from the East Indies, under the Name of Gum Kuteera, and of which a large Quantity has been lately imported into this Kingdom. By Mr. JOHN COWIE\*.*

To Mr. TAYLOR.

SIR,

THERE is a gum produced in several parts of Oude and the adjacent provinces, so nearly resembling gum tragacanth, as to have been taken for it by many; and large quantities of it, of late years, have been imported into Europe, under this mistaken opinion. But it is now well ascertained, that this gum (which in the country language is called kuteera) is the produce of a particular tree, of quite a different species from the

Gum resembling g. tragacanth,

\* Society of Arts, Vol. XXI.

thorny

but not so glutinous, or applicable to the same uses.

thorny bush which yields the tragacanth; and being imperfectly soluble, and possessing but little of a glutinous nature, renders it inapplicable to the purposes for which gum tragacanth is used. I am nevertheless of opinion, that this gum might be found serviceable, in one way or other, to some of the arts and manufactures, and am therefore induced shortly to describe it, and the use to which it is applied in India, with a view of exciting any of your ingenious correspondents to make experiments on it, and determine its real value, or absolute inutility.

Description of gum kuteera.

The kuteera is in loose wrinkled drops or pieces, void of smell and taste, of a whitish colour, and mostly transparent. In water, it slowly forms itself to a pulp or jelly, and is nearly tasteless; but, if pounded well in a mortar, and then boiled in water for fifteen minutes, stirring it all the time, it will be found *completely dissolved*. A tea-spoonful of this powder gives to three pints of water the consistence of capillaire. Might it not in this state be of service to painters and artificial florists, or even used to give a gloss to silks? The natives of India make a varnish by mixing kuteera with other gums; and I have been told, they likewise make use of it in the printing of calicoes. It is the principal ingredient in a medicine they give to their horses in certain diseases, and in this respect is of very general consumption.

It promises to be useful in other respects.

Large quantities are now in the India warehouses.

I shall only add, that many tons of this gum are now lying in the East India Company's warehouses, totally unsaleable, or that will not sell for more than the first cost in India; and if this notice shall be attended with beneficial consequences, I shall derive great satisfaction in the reflection of having served the interest of the speculators in this drug, amongst whom I could formerly number myself; and I believe I was the first importer of it into this country.

I am, SIR,

Your most obedient Servant,

JOHN COWIE.

Finchbury-square,  
25th May, 1802.



## XVII.

*Galvanic Experiments with Ice, and a Method of rendering the Electric Attraction of the Pile very evident. By S. P. BOUVIER, of Jodoigne, Member of the Society of Emulation, of Natural History and Physics, at Brussels\*.*

I AVAILED myself of the first frosts this year to make some experiments on ice, both as an interposed substance, and as an exciter and conductor.

*Experiment 1.* A pile was formed of eighty pairs of plates of zinc and silver, and as many very thin disks of ice. It not only did not give the slightest shock, but did not excite the least taste on the tongue, or produce the smallest luminous effect. I left it standing for several hours, but found no difference in the result.

*Exp. 2.* I arranged ninety disks of ice in pairs with as many crown pieces, and placed between the pairs pieces of pasteboard impregnated with a solution of salt. This combination produced no more effect than the preceding.

*Exp. 3.* The same number of disks of ice were paired with as many of zinc, and wet pasteboards interposed as before, with the same result.

*Exp. 4.* I made a pile of a hundred and twenty-eight pairs of zinc and silver, interposing pasteboard impregnated with a saline solution. This pile gave such strong shocks, that they extended to the shoulders with violence. On forming the circle of this pile with the hands armed with little pieces of ice, all the power of giving a shock was intercepted. I then took a piece of ice in my mouth, and brought it into contact with the summit of the pile, while with my hand I touched its base; but no taste was perceived on my tongue.

The thaw prevented my pursuing this inquiry any farther. If the frost return, I will pair disks of silver with cast disks of muriate of lime, of pure potash, and of sulphur of potash, in order to produce in the first two cases an action of physical solution, and in the last a chemical action, after the manner of Davy.

A pile of 80 pairs, separated by thin disks of ice, produced no effect.

A pile of 90 pairs of ice and silver, with wet pasteboard interposed, had no effect. Ice with zinc was equally inactive.

128 pairs of zinc and silver, separated by wet pasteboard, gave strong shocks felt in the shoulders; but if touched with a bit of ice held in the hand, nothing was felt. It could not even excite taste, when the communication was made with ice. Farther experiments proposed with muriate of lime, pure potash, and sulphuret of potash.

\* Journal de Chimie, par Van Mons, for January 1803, No. XI. p. 52.

On a pile of 140 series, a delicate magnetic needle was suspended; on approaching the hand to it, an oscillatory motion took place, but the magnetic attraction appeared to be stronger than the electric.

*Exp. 5.* On a pile of 140 series of zinc, silver, and paste-board, impregnated with a solution of salt, I placed an iron pivot, and on this pivot a very delicate and sensible compass needle. The friction was almost nothing. I first applied one hand to the base of the pile, and I carried the other near to the needle. I observed a slow and oscillating approach; but the needle was incessantly invited to resume the magnetic direction, whence I inferred, that the polar attraction exceeded the electric. I tried then to render the transmission of the fluid more easy, by making it glide over a metallic conductor, as a brass wire; but the result was the same as when I made the fluid pass through my arm.

A common pin, suspended in the same way, was attracted by a wire communicating with the pile, at the distance of some lines.

*Exp. 6.* To the magnetic needle used in the preceding experiment, I substituted one made of a pin, the movement of which also I rendered very free. I approached this needle with the extremity of a brass wire that communicated with the base of the pile, and I perceived it turn with rapidity, at the distance of a few lines, to rush against the wire. A similar communication established through the arms afforded the same result.

A silver thread, suspended from a knitting-needle, placed on the summit of the pile, was attracted by the finger.

*Exp. 7.* I placed on the summit of the pile an iron knitting-needle, bent, and suspended to it a silver thread, as it is called, used for embroidery. I completed the circle with one of my fingers, approaching the thread gradually with it. At a certain distance the thread applied itself to my finger, and remained adhering to it, though it was perfectly dry.

When the communication was made with a brass wire, vivid sparks were given out, the silver was oxidized, and part of it melted. The same effects took place repeatedly.

*Exp. 8.* I repeated the preceding experiment, only forming the communication by means of a brass wire. The attractive effect equally took place, and very vivid sparks were given out between the wire and the thread, which oxidized the latter over all its surface, and melted a part of it several lines in length.

All these experiments were repeated a great number of times, and always with the same success.

A silver thread, suspended from a wire fixed to the summit of the pile, was attracted by one hand, when the other was dipped

*Exp. 9.* In the upper disk of a pile of 97 pairs of zinc and silver, I fixed an iron wire, and suspended to it a similar silver thread. At the foot of the pile was a communicating basin, in which was a solution of salt. Dipping one hand into the basin, and bringing the other near the thread, this immediately came and stuck to it. When I took my hand out of the basin,

the



the thread immediately quitted the other, but stuck to it again when I redipped my hand into the basin; and so on alternately.

*Exp. 10.* I repeated the preceding experiment, fixing a brass wire, in order to obtain more firmness in the contact with the thread, into a block of lead, placed on the table on one side of the pile. I bent the wire into a crook, and suspended from it an embroidering thread, which I placed at a small distance from the knob of the pile. As often as I held one of my hands wet against the block of lead, and dipped the other into the water in the basin, the thread applied itself to the button of the pile, and stuck there; but it quitted it as soon as I interrupted the communication by withdrawing either hand. This experiment, repeated more than fifty times following, was constantly attended with the same result.

into a basin of salt and water communicating with the pile.

A silver thread, suspended from a wire fixed in a block of lead on the table, was attracted by the knob of the pile, when one hand was dipped into the basin, and the other, wetted, touched the lead; but on removing either hand, the effect ceased.

### XVIII.

*Experiments on the Activity of a Galvanic Pile, in which thin Strata of Air are substituted instead of the Wet Bodies. By Mr. DYCKHOFF\*.*

MR. RITTER has observed †, that all substances which, when interposed between the constituent parts of the pile, excite electricity, are active only so far as they contain moisture; and he asserts, that it is impossible to construct an active pile without the intervention of a wet substance. I take the liberty to oppose to this assertion an experiment, by which it is contradicted. I constructed a pile with disks of copper and zinc, and little bits of thin green glass, about the size of a lentil, three of which I placed triangularly in the intervals that separated the metallic plates. Thus between each pair of metals I had a thin stratum of air instead of a wet substance; and the following were the results:

1. A pile of ten pairs, tried by the condenser, affected the electrometer as powerfully as a common pile of five pairs,

\* *Voigt's Magazin fuer den neuesten Zustand der Naturkunde*, Vol. IV. p. 791.

† *Intelligenzblatt der allgemeinen Litteraturzeitung*, 1802, No. 193.

Ritter asserts, that moisture is necessary in Volta's pile.

But a pile of copper and zinc in pairs, separated by bits of glass, so as to interpose air alone, exhibits electricity;

though only in a degree equal to that of a common pile half as high.



Damp air did not answer so well as dry, apparently.

The electricity was transferrable to the Leyden phial.

The weakness of the electricity perhaps owing to the roughness of the plates.

2. The action remained invariable as long as the air continued dry; but damp air appeared less favourable to the pile.

3. The same degree of electricity was communicated by the pile to a Leyden phial.

The plates were only three inches in diameter. As they had already been used for a pile in which wet substances were interposed, and were much corroded, I took off the oxidized portions by a file; but the plates still remained rough and uneven; and to this circumstance I ascribe my not obtaining a higher degree of electricity. I have yet had no opportunity of repeating the experiment on a larger scale; but the result I obtained appeared to me sufficient to render the assertion of Ritter at least questionable. I see no reason why glass, thin strata of sealing wax, or the like, should not have the same effect on Volta's pile as in the condenser. For the experiment to succeed, it is necessary that the bits of glass should be as thin and even as possible; for, if the stratum of air be too thick, it is natural that the electricity, which is excited in such a feeble degree by the contact of the two metals, should remain inactive on the adjacent metallic pairs.

No reason why glass, &c. should not act in the pile, as in the condenser.

The glass must be as thin and even as possible, that the stratum of air may be so.

## XIX.

*Extract from a Letter from Mr. J. B. VAN DEN SANDE, Professor at Luxembourg, on the Efflorescence and Decomposition of Glass Tubes\*.*

Oxidation of white glass not uncommon.

Old barometer-tubes exposed to flame, turned white and opaque;

and the same if simply heated.

I HAVE lately had an opportunity of observing at leisure the common phenomenon of the calcination of white glass. I exposed to the flame of an enameller's lamp some old barometer tubes that appeared not to have any defect, in order to bend them into syphons. Scarcely had they come into contact with the flame, when they turned white and opaque. One of the tubes being wetted, I placed it on a chamber-stove to dry; and before it was heated, it was covered with a whitish efflorescence. Heating the others produced on them the same effect. I then removed the efflorescence, replaced the tubes on the stove, and they became white afresh. By continuing the ex-

\* Journal de Chimie, par Van Mons, IV. p. 232.

periments, I reduced an eighth part of the glass to a very fine  $\frac{1}{8}$  of the glass powder; after which the tubes were opaque, and had lost their <sup>thus effervesced,</sup> polish.

The greater part of this efflorescence exhibited in the microscope rhomboidal parallelograms; the rest triangles, most <sup>in rhomboidal parallelograms or triangles.</sup> of which were isosceles.

Having poured nitric acid on a portion of the effloresced matter, it dissolved 0.20. One part of this solution afforded, <sup>The efflores-</sup> on evaporation, crystals of nitrate of soda: the other afforded <sup>cence was soda.</sup> no precipitate with potash.

These tubes were become good conductors of the electric fluid; which led me to presume, that their decomposition was <sup>These tubes had become good conductors of electricity; the decomposition therefore owing probably to humidity.</sup> occasioned by the humidity of the place where they were kept; which humidity, after it had penetrated the surface of the glass, imperceptibly insinuated itself into its pores. This efflorescence, however, depends much on the quality of the glass; for we may observe in the same glass frames, with a fourth or fourth-west exposure, some panes calcined to such a <sup>But the efflorescence depends much on the quality of the glass.</sup> degree as to become entirely opaque, while others retain their transparency, and are perfectly intact.

## XX.

*On the Criteria or due Discriminations of Cyder Fruit.* By  
THOMAS SKIP DYOT BUCKNALL, Esq.\*

To CHARLES TAYLOR, Esq.

SIR,

HEREWITH you receive a paper on the criteria or due <sup>On cyder fruit and the making of cyder.</sup> discriminations, by which the valuable cyder fruit may be ascertained, referring to my last and preceding Papers on Varieties.

Report says, cyders are not well produced out of the cyder countries. Were it said they are not well made, the assertion would be just; but it is a mistake to suppose that cyder cannot be made in perfection out of those situations. Only choose the proper soil for the fruit, see it ground down to a perfect smoothness, rind, pulp, kernels and all; duly fermented.—Tun the liquor at the exact point of time with regard to settling, which

\* Soc. Arts, Vol. XXI.



On cyder fruit  
and the making  
of cyder.

is well understood in the West, and not attended to elsewhere; and do not rack it too much. Keep the liquor in quantity to a good age; and I take the liberty of maintaining, that there are places in Hertfordshire capable of producing as good and fine cyder as any in Herefordshire.

I was once asked what are the impediments to a more general fine crop of fruit? My reply was, the coldness and uncertainty of the climate, injudicious planting, with inattention and neglect in those who have the management of it. As for planting and guarding against cold, enough has been said on that subject through the whole of my treatises. It has been observed the cyders were better one hundred years ago than they are now. That may have been the case; and I should account for the difference as follows:—When the gentry and superior yeomanry of the country depended on barley and fruits to make an agreeable beverage for their friends, it is natural to suppose that they personally superintended the business themselves, and received great satisfaction when their labours were attended with success; whereas, when Port wines became the luxury of the country, cyder, of course, naturally fell into disuse at the better tables. Hence the masters of families were no longer zealous to produce fine potent liquor, and giving it as cyder, that being assigned to inferior uses.

Undoubtedly, where cyder is the general drink of the country, great quantities must be raised for that purpose; and it is done at so low and easy a rate, as to be sent from the mill to the neighbours' cellars under three half-pence the gallon. What I am attempting is, to be able to produce, *at a certainty*, a fine generous liquor, such as may do credit to the name of the cyderist, and which will depend much more upon attention than is generally imagined.

After having expatiated on the old and new varieties of the valuable cyder fruits, in the last volume of the Society's Transactions, I have nothing farther to offer for the purpose of closing the Orchardist, than to present my best respects to the Society, and give them the marked criterions as follow:—

My friend, Thomas Andrew Knight, Esq. has made good discriminations in his Tract on the Apple and Pear. The Somerset report has further extended the subject.

I say, choose an apple naturally small, of a whitish colour, somewhat tinged with red; of a fine yellow pulp, with a cer-



tain degree of astringency, and as much saccharine matter as nature is disposed to produce; then observe if the cells are large, and full of ripe kernels; and further still, know whether the blossoms are patient of cold, and the fruit ripens well.

On cyder fruit  
and the making  
of cyder.

Such an apple, particularly if a new variety, properly ground down and duly fermented, must make good cyder; and both as to profit and use, be valuable in any neighbourhood, or as an article of trade. The foundation is now so well laid, in the seedling beds in the county of Hereford, that within five years there will be more than one hundred new valuable varieties produced. I last autumn saw two most beautiful new apples, of the first year's growth, upon the Grange estate, and which decidedly obtained the Hereford premium: they were seedlings of high promise. There is an emulation among gentlemen of that part of the country, which does them great credit.

Dr. Symonds gives several of these discriminations; and in addition says:—The flavour of a good cyder-fruit cannot be mistaken by a man conversant with apples, though difficult to be described; but above all, he recommends to choose apples, the rinds of which have follicles, or cells, containing large quantities of essential oil, more particularly to be noticed in the old fire, golden pippin, and paufon, and from which Dr. Symonds conceives the cyder in a great measure derives its flavour. It is to be observed, that the old paufon, woodcock, and red musk, are generally large apples. The old scorched-harvey is an exception as to the yellow colour; that being white, with a brown skin. There are other exceptions; but the material criterions are here enumerated according to the received opinion.

Had I formed these distinctions when I was a boy, I should then have said, take any apple bordering on the golden rennet or golden russet, and it will make good cyder, in consequence of supposing that the golden colour is of service. We had a fine apple, the royal pearmain; it was more flat and large than the Seville orange; it had a thin skin and quick taste, with much sweetness, and made fine cyder: but I presume that variety is now over, as I have not seen one of them for many years.

In the October of 1801, which I spent in Kent I was shown a new apple. The tree is handsome, and the fruit has most of the

On cyder fruit  
and the making  
of cyder.

the criterions, but does not ripen well, which is a great defect in cyder making; we named it *The Bland of Hartlip*. It has the macula, or follicles in the skin, more visible than on the golden pippin. I intend examining it with attention at a future period, as it is not possible to form a certain judgment on it for some years. My reason for mentioning the Bland of Hartlip is, to prove that the idea of searching after new varieties has made a general impression throughout the kingdom.

It was the custom formerly, if a new apple had many good points, but ripened late, to preserve the tree as a new variety, imagining that, as it advanced in age and acquired strength, the fruit would ripen the sooner, and consequently the cyder be richer. I should add to it, lay the land dry, and spread plenty of manure. These attentions would much accelerate the wished-for object of improving the liquor, as well as increasing the quantity of fruit.

I remain,

DEAR SIR,

Your obedient Servant,

THO. SKIP DYOT BUCKNALL.

Hampton-Court, Nov. 14, 1802.

## XXI.

*Concerning the Steam Engine from Mr. Blakey, by a  
Correspondent. B.*

To Mr. NICHOLSON,

London, March 20, 1804.

SIR,

Introductory  
letter.

SOME time about the year 1776, I met with an engineer of the name of Blakey, in Holland, of whom Ferguson the philosopher and several other ingenious men have spoken in high terms. I do not know whether there is any history of him or of his works; but as many of his contemporaries must still be living, I should hope that this notice may produce some information through the medium of your Journal. I have lately met with a pamphlet by this author, intitled "*A short historical Account of the Invention, Theory, and Practice of Fire Machinery*," printed at London, in the year 1793, without book-seller's

seller's name. It chiefly consists of short notices of the author's own operations, with academical testimonies; and I understand from private information, that it was published at that time by Mr. B. in indigent circumstances, with a view to obtain subscriptions for a large treatise, which has not since appeared. Some of your correspondents may probably favour us with an account of the disposal of those writings since his decease. I have extracted the outline of history of the steam-engine from the beginning of his pamphlet, which, if you think will be acceptable to your readers, is very much at your service.

I remain, Sir,

Your obliged Reader,

R. B.

IN the beginning of the seventeenth century, a modern philosopher conceived the notion of making use of fire and water to obtain motion. This was Brancas of Rome, who contrived a stamping-mill to be worked by steam, coming from a large æolipile, as may be seen in his twenty-fifth plate. We are obliged to him for a number of ingenious inventions, which he dedicated to M. Canci, governor of Loretto, in 1628, and published his work at Rome the year following.

Brancas first applied Steam to mechanism; or proposed it, 1627.

Thirty-four years after, the Marquis of Worcester published a little treatise, under the name of "Century of Inventions, printed in London, 1663;" he gave them as if he had tried them, and proposed other engines of great utility, which only wanted encouragement from government: "Several were nothing but projects," says Desaguliers, "but, where he is more explicit, there is one for raising water by means of fire."

Marquis of Worcester, 1663.

In the acts of Leipzig, 1690, p. 410. is to be seen an invention, intitled, "A new manner to procure a considerable quantity of power with little expence, by Dr. Papin." "In this new engine," he says, "of which we have given a description in the acts of Leipzig, 1688, and the intent of which was to obtain a new method of making use of gunpowder as a power to give motion, the chief fault was, that the gun-powder, in the under part of the tube, did not entirely fill

Papin, 1690,



proposed a piston  
and condensa-  
tion.

fill the capacity of this tube, so as to have no air under the piston \*." Papin continues to give reasons like a man of great understanding; but as there must be plates and descriptions to explain the whole, I shall say no more on the subject, but only add, that the doctor did his best to obtain his end in another manner. He shews that, with "A little water changed into steam by means of fire, we can have an elastic power like air; but that it totally disappears when chilled, and changes into water; by which means he perceived, that he could contrive a machine in such a manner, that, with a little fire, he would be able, at a small expence, to have a perfect vacuum, which could not be obtained with gun-powder." Among various contrivances, this appeared to answer the best. In the work itself a plate of this contrivance will be given, which, however, is not to be compared to what has been invented since Papin, by men who had never heard of him, and were altogether much inferior to him in theoretical and practical philosophy.

Capt Savery,  
1699.

In 1699, a remarkable year for the honour of English ingenuity, Captain Savery shewed to the Royal Society an engine which raised water by means of the expansion and condensation of steam; he published a little treatise, which he named "The Miners' Friend;" the description of this engine is to be seen in Harris's Dictionary of Arts, &c. printed in London, 1704. This learned man says of Savery's engine, "that with little art it can be kept in action as long as one will, and that it cannot be hurt but by stupidity and negligence." Harris looked on this invention of Savery's to be one of the most ingenious combinations that had ever been made in hydraulics, and I may add, that it is surprising that a seafaring gentleman, in all likelihood little used to the working of metals, should have brought his invention to the perfection he left it in.

\* It is seen that Papin is the first that thought of a piston to be applied in fire machinery, though it was but in a tube about two inches diameter, yet it shewed the utility of pistons, which the ancient Greeks knew as well as the moderns, being one of the most simple inventions in hydraulics, as may be seen in Vitruvius, speaking of the pumps of Ctesibus for the forcing water to a great height.

In the memoirs of the Academy of Sciences at Paris, page 112, 1699, is to be seen an account of Mr. Amonton's, who shewed the drawing of an engine, in which fire heated air, which was to force up water on a wheel, to make it turn. We suppose he understood his theory; but to me it appears impracticable, though it must be said that Monsieur Amonton's notions are ingenious and useful for those who desire to know to what degree the expansion of air, for procuring power, can be brought.

Amonton's  
rotatory engine,  
1699.

There is also seen in the same Academy's works, "Mr. D'Alefmé 1705 D'Alefmé's proposal to raise water by means of steam, proceeding from hot water; in a machine (a sort of æolipile) in which the elasticity of the steam makes the water spout to a great height, so strong is that expansive power." August 5, 1705.

In 1707, the famous Papin published, at Cassel, a treatise on a new machine for raising water by means of fire; he thought his manner was better than Savery's. He says, "All the world shall know that it is the Landgrave of Hesse to whom this invention is due, and by him it was brought to the perfection it is in, though many obstacles were found in the execution."

Papin, 1707.

I cannot help remarking, what a pity it is that such an ingenious man as Papin should take so much pains to acquire the reputation of being the first in the invention of fire-machinery. To appear less partial, he takes from his own merit to flatter the landgrave, who certainly did not understand those matters so well as he: but, notwithstanding the consideration that is due to a sovereign and to the celebrated Papin, I make bold to say, that this engine was far from having the merit of Savery's; and I can say more, experience has made me believe that no man has seen the doctor's engine work a couple of hours, as may be conceived from the difficulty he found in making steam, and encreasing its elastic power in his receiver. Papin has nevertheless given us demonstration in upwards of forty pages of calculations, which prove his understanding and learning. This machine is in my academical works.

In 1710, Thomas Newcomen, ironmonger, and John Cawley, glazier, at Dartmouth, contrived to work a piston in a cylinder, by the weight of the atmosphere only, when they had formed a vacuum in their vessel; but it took them

Newcomen and  
Cawley's atmo-  
spheric engine,  
with a piston,  
1710.

much

much time to bring such a complication of apparatus to perfection, for which they were criticised more out of jealousy than reason; however, they brought their engine to perform by hand as well as possible for that time, much to their honour, as well as to that of a country boy whose name was Humphry Potter, and who, to give himself time to play, contrived tackles to make the engine work itself.

The bringing this engine to perform as it did, proves that, when nature is ingenious, it finds out the way to come to its ends without the humdrum pedantry of schools. An iron-monger, a glazier, and a country boy, brought to perfection the lever fire-engine, which all the universities and academies of Europe had not the least notion of before these ingenious mortals appeared. This discovery has not been improved but in accessories, and by no means as to principles, as these are more simple, and can work with less fuel than those so much vaunted, if care is taken in making them.

As soon as these happy inventions came to light, Beighton, another ingenious man, slept forth and made tables for the dimensions of vessels, in proportion to the weight of the atmosphere, which proportions are followed to this day. This has made the world believe that the whole art of fire-machinery is concentrated in Newcomen and Cawley's lever-engine.

Defaguliers,  
1718, and  
Moura, their  
improvements.

In 1718, Doctor Defaguliers made an amendment (according to him) on Savery's engine; but his method was not followed, as it caused the death of the manager; which must often happen when the dimensions are in the manner he contrived them.

After this engine, Mr. Moura, a Portuguese gentleman, invented some geers to make an engine, on Savery's principles, to work itself: they were ingenious, but too complicated to be put in practice.

The making a fire-engine for York-buildings gave an opportunity to improve as to the manual part, and to be less expensive.

Large cylinders,


Experience in making fire-engines has brought Mr. Darby, of Colebrook-dale, to make the cylinders large enough to have a vacuum in them, equal to the weight of 80,000lb. of atmosphere, ten and fifteen times in a minute. This perfection of workmanship has procured to England a great ex-

portation



portation of cylinders, pipes, &c. to many parts of Europe and America. At Liege, and in the Netherlands, they used to have their cylinders from England; but, being very expensive from the duties and carriage, &c. the industrious Poisson, at Liege, with his small foundry, found means to make cylinders of three pieces screwed together, which he bored, and made them serve as well as if they were of one piece. I have seen one work as well as can be desired.

In 1752, Monsieur Rivatz, a Swiss gentleman, made a fire-engine on Savery's principles for the Chevalier Nugue, at Nogent sur Marne, two leagues from Paris; the apparatus to make the engine work itself was of his invention, and performed very well; his boiler, however, being out of proportion, the man who attended the work was much hurt by the explosion of the steam, which discredited the machine, though well contrived in every part except the boiler.

 Next follows the Account of the Author's own Works.

## XXII.

*Observations and Communications on the Dry Rot in Timber, made to the Society for Encouragement of Arts\*.*

THE mischief arising in buildings from that decay of the timber and wood-work, known in general by the name of the Dry Rot, has been, and yet continues so great, as to demand every attention for its prevention. In the XIIth Volume of the Transactions of the Society of Arts, published in the year 1794, will be found some valuable facts, furnished by Robert Batson, Esq. of Limehouse, respecting the methods he took to prevent this evil, in one of his rooms greatly affected by it. The plan he adopted was, to charr the ends of his timbers, to take away the infected earth to the depth of two feet, and to fill up that space with anchor-smiths' ashes, or ashes from a foundry, before his flooring-boards were laid. On the 15th of May, 1794, which was upwards of six years after the flooring was laid, as above mentioned, a minute examination of the boards, wainscot, and timbers, was made in the presence of a Com-

Introductory remarks by the Society of Arts.

Robert Batson, Esq. his method of curing the dry rot.

\* Extracted from their Transactions, Vol. XXI.

mittee of the Society, and they were all found entirely free from any appearance of the Rot. To investigate the matter more fully, a further inquiry has been made in June, 1803, and an answer received, that there has been yet no appearance of the Dry Rot there; the Society, therefore, think it may be of consequence to notice the fact, and to insert, in the present Volume, some other papers with which they have been favoured upon the subject. They contain many hints deserving public attention, and which will doubtless tend to check the progress of this evil.

*Letter from BENJ. JOHNSON, Esq. on the same Subject.*

TO CHARLES TAYLOR, Esq.

SIR,

Account of a  
pew very much  
affected by the  
dry rot.

SOME time between 1771 and 1773, I went, at the request of a friend, to the Chapel at the Lock-Hospital, through curiosity, to examine a pew there, that had frequently been repaired for damages by the dry rot.

It is a plant.

After a close investigation, we found that it was the operation of a plant, whose leaf resembled that of the vine. Wherever it had touched, the effect of its poisonous quality got through the wood to the paint, which I have seen a mere skin. I proposed to cover the floor with bricks, laid in mortar, which was accordingly done. I called twice since, the last time about seven years ago; and have reason to think that it had never appeared again.

Destroyed by covering the  
ground.

The next opportunity of examining it carefully was at Mark-Hall, in Essex, the seat of Mr. Montague Burgoyne. In a parlour there were three pillars of about ten inches in diameter, the outwood of which was between two and three inches thick. Two of them were eaten through in less than seven years, from the bases, about two feet upward, within the hollow, and were as rotten as if it had been the effect of a hundred years standing. Mr. Montague Burgoyne's gardener was a botanist: we found the plant where I directed him to search for it; and he said it was the *Boletus Lachrymans*. \*

Dry rot at Mark-Hall.

The plant found.

At

\* Some authors call it a parasitical plant; and it is sometimes to be found with the willow and fallow tribe, but this is not to the purpose.

At another time, I saw it in a house at Whitehall, built by Sir John Vanbrugh, whose nephew then lived in it. The house is, I think, only two stories high; the plant had ascended to the upper story, committing devastation on the wainscot all the way. It will destroy half-inch deal wainscoting in a year.

Another instance at Whitehall,

I have had it twice in houses I inhabited, one in Suffolk, and the other in Gloucestershire. I bore with the first; in the other case, I undertook, and did stop it effectually.

The cause is from the floor being laid on the earth, which has been, where I have observed, of a gravelly or sandy loam. The moisture from a water-course at hand, or a North aspect, where the outer wall stands in a garden-bed, so that the rain percolates, are great encouragers: it requires moisture.

It is caused by communication with moist earth;

It never rises in the middle of the floor; because, if the seed were there, it could not germinate for want of air; but it is easy to suppose, that after the floor is shrunk, an air may be created between that, and the vacancy between the wainscot and outer wall sufficient for the purposes of vegetation.

with access of air.

I saw an instance, last summer, in the house of a friend, a student in botany. He was surprised when I told him, it was a visit from a plant; but so it proved, and always is, and ever was so; nor does it originate from any other cause.

In my own case, I removed the original soil near the part affected, and supplied its place with sand. I then placed pieces of tile; on those I laid mortar, and tiles over them, pushing them under the wainscot, so that it had no communication with the joists or floor. Pillars, in like manner, should be kept from the earth.

Process of cure.

In laying a floor upon the ground, I should take away the earth for a foot in breadth, and four inches in depth, all round the walls; and place the ends of the joists in mortar, covering them with tiles pressed under the floor and wainscot, quite to the outward wall. Iron or tin plates would do, but are not so cheap as mortar and tiles.

purpose. Till within a few months I have never been without some leaves of the plant. For many years they appear exhausted and dead, and soon crumble into dust; but I suspect that fresh wood attracts a fresh growth from the root;

This



The plant does not run except along wood.

This plant has no adhesive powers, but in contact with wood. If it could pass over brick or mortar, it might be seen to spring from the cellars, and infect half the houses in the kingdom.

In short, the wainscot is to be kept free from contact with the joists and floor; and I believe it cannot be better effected than I have described.

I am, SIR,

Your obedient Servant,

Dec. 21, 1799.

BENJAMIN JOHNSON.

To the Secretary.

(To be continued.)

### SCIENTIFIC NEWS.

*Society of Agriculture, Commerce, and Arts, at Boulogne-sur-Mer.\**

THE following questions proposed by this society are noticed as of general and not local import.

What are the nature of marles, their species, and the best modes of using them?

1. It is generally acknowledged, that marling is one of the most important means of improving land; accordingly the society will bestow a prize on the memoir, in which the nature of marles, their various species, and the most advantageous mode of using them, according to the difference of soils, shall be best unfolded.

Their external characters necessary to be known.

The author will be particularly careful to point out to farmers external characters, that will enable them easily to distinguish each species of marle.

Sheep liable to the vertigo or dunt. In France, 900,000, or 950,000, that is 3 or 4 in 100 die of it yearly.

2. Sheep are subject to a disorder known by the name of *vertigo*, or the *dunt*. The number that die of this disorder may be estimated without exaggeration, at three or four in a hundred annually. If then there be about thirty millions in France, we may reckon, that nine hundred, or nine hundred and fifty thousand are thus lost every year. It is occasioned by a hydatid formed in the head of the sheep. This hydatid

\* *Magazin Encyclopedique*, No. 3. June, 1803. p. 401.

contains an extremely limpid water, and a white substance, divided as if into grains, which have been found by the microscope, to be real worms. It appears, that to cure this disorder, the head of the sheep must necessarily be opened, and the hydatid extricated: but this operation has hitherto appeared so hazardous, and has been attended with so little success, even in the veterinary school at Alfort, that the Society has deemed it highly important to the science of agriculture, to offer a prize to the person who shall point out the best methods of treating the vertigo of sheep, and curing them perfectly of this disorder. He will take care that his method be supported by experiments made with accuracy, and confirmed by unquestionable testimony.

Caused by a hydatid in the head.

Its extraction dangerous.

What is the best method of treating it?

The prizes will consist of medals, which the society will deliver at the public meeting, on the 28th of April, 1804.

*Crystallization of Lime by Tromsdorff, denied by Berthollet.*

IN the first volume of the octavo series of this Journal, p. 302, is inserted "A method of crystallizing lime, by Professor Tromsdorff," translated from the Journal der Pharmacie, Vol. IX. part I. p. 108. The same process is also in Van Mons's Chemical Journal, No. II. According to which, muriate of lime is to be boiled with one fourth or even less of caustic lime, and the fluid is to be concentrated. Afterwards, by slow evaporation, long slender crystals of *caustic lime* are obtained, which are to be freed from the adhering muriate by ablution in alcohol.

Error respecting the crystallization of lime.

This result is denied by Berthollet, who, in his *Essai du Statique Chimique*, Vol. I. p. 350, says, "I have repeated this experiment, and have ascertained that these crystals were *not lime*, as has been announced, but *muriate of lime with an excess of lime*." He continues, "if these crystals be treated with water, other proportions are established; the part which dissolves is the muriate, which retains but a small excess of lime, and the portion which is not dissolved retains a greater excess of lime; by adding water, successive separations may be obtained, and the proportions will depend on the relation of the dissolving force to the resistance of the cohesion."

*Conversion*

*Conversion of Pieces of Cloth impregnated with a Solution of Salt into Soap of Wool, by the Action of the Galvanic Pile.*

By BRUGNATELLI.

Cloth wetted with salt and water, used in the galvanic pile, is converted into soap of wool; the muriate being decomposed by the zinc, which is oxidized, and the soda combining with the wool.

I HAVE examined the singular alteration produced in the bits of cloth wetted with a solution of salt, interposed between the disks of copper and zinc arranged in the pile, and I have found, that they are at length converted entirely into soap of wool.

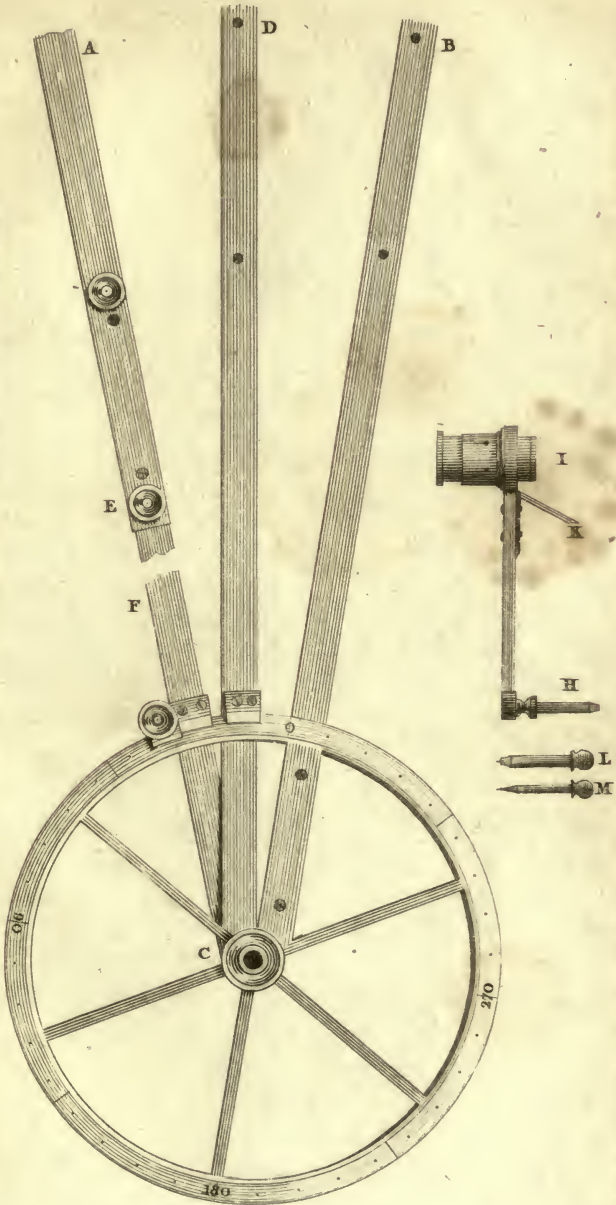
On this occasion the muriate of soda is decomposed by the zinc, that is continually undergoing oxidation, and the soda, separated by this decomposition, attacks the wool, and forms with it soap.

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THE Work of Mr. Parkinson, of Hoxton, on the organic remains of the former world, is in considerable forwardness. The first part on the fossils of the vegetable kingdom, illustrated with coloured plates, in quarto, is proposed to be published on the first of June next.



Station Pointer by Joseph Huddart Esq. F. R. S.

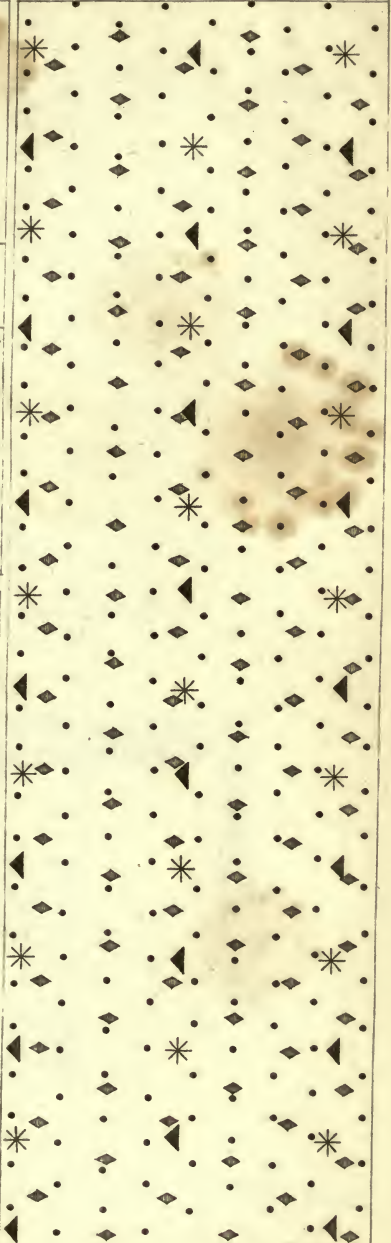
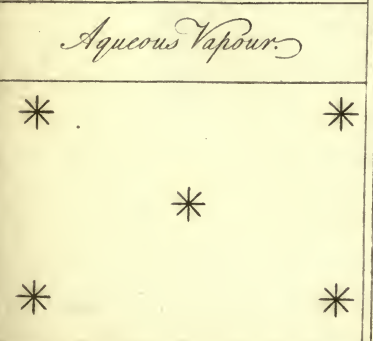
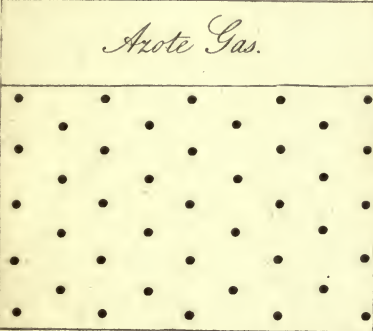
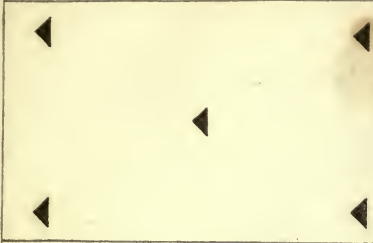




*Mr. Dalton's Theory of the Gasiform State.*

*Carbonic acid Gas.*

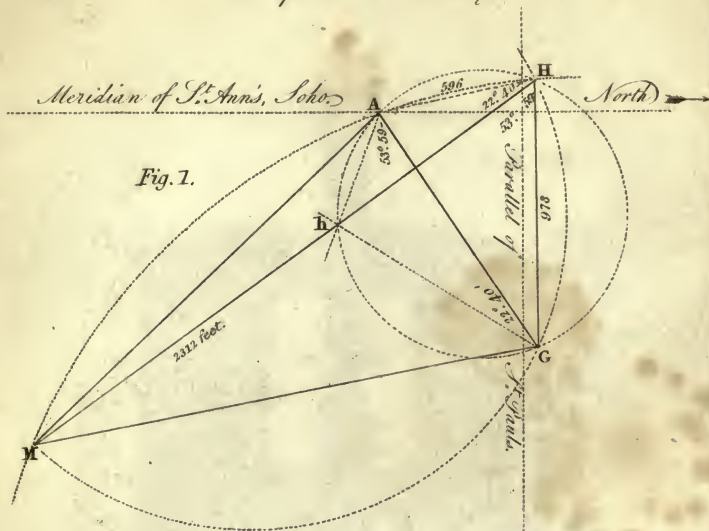
*Compound Atmosphere.*



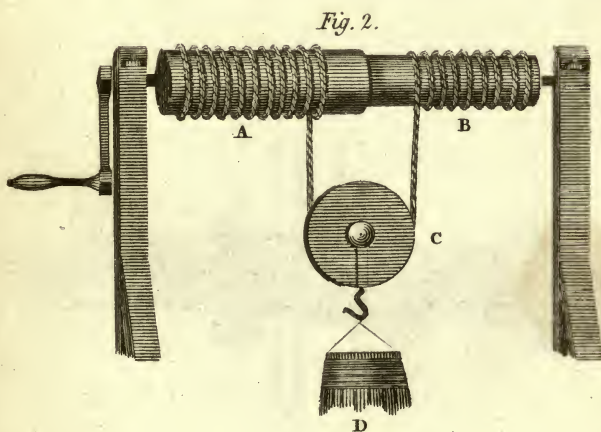




*Determination of Positions, by  
observation of three known objects.*



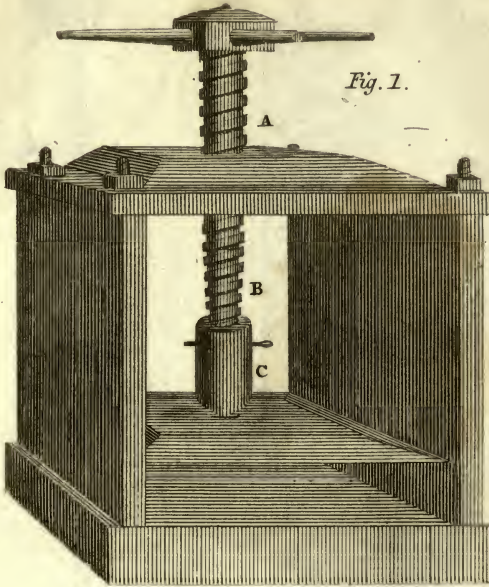
Winch & Barrell, (double)



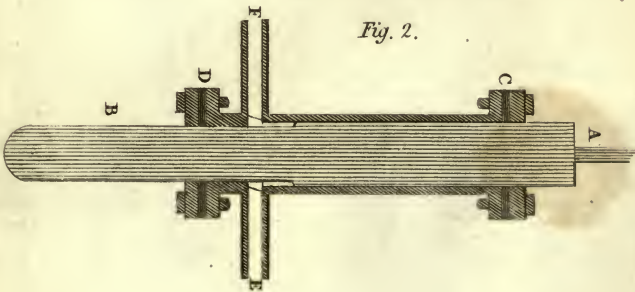




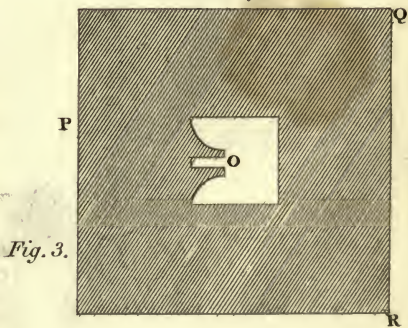
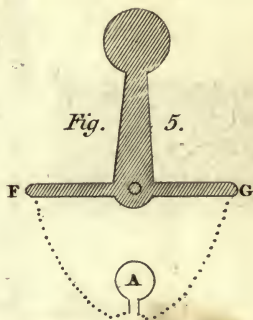
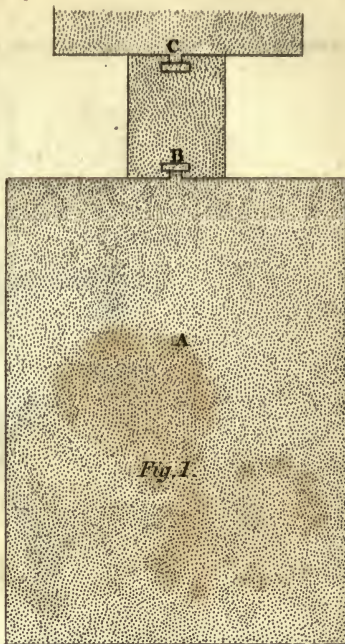
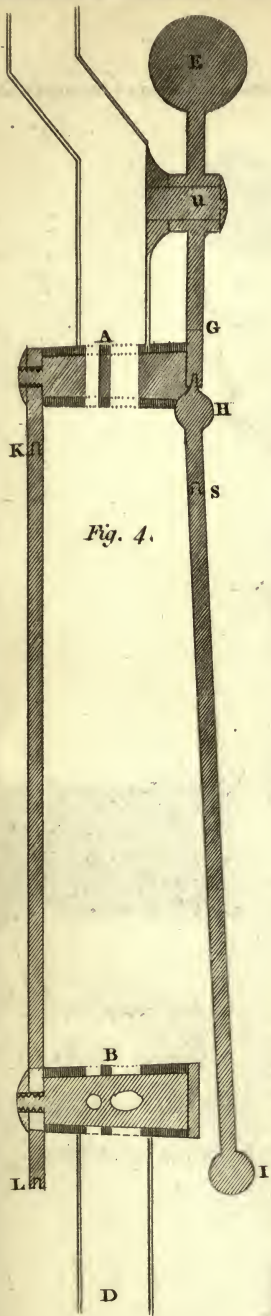
*Increase of power by a  
Double Screw.*



*Pump with double Piston.*





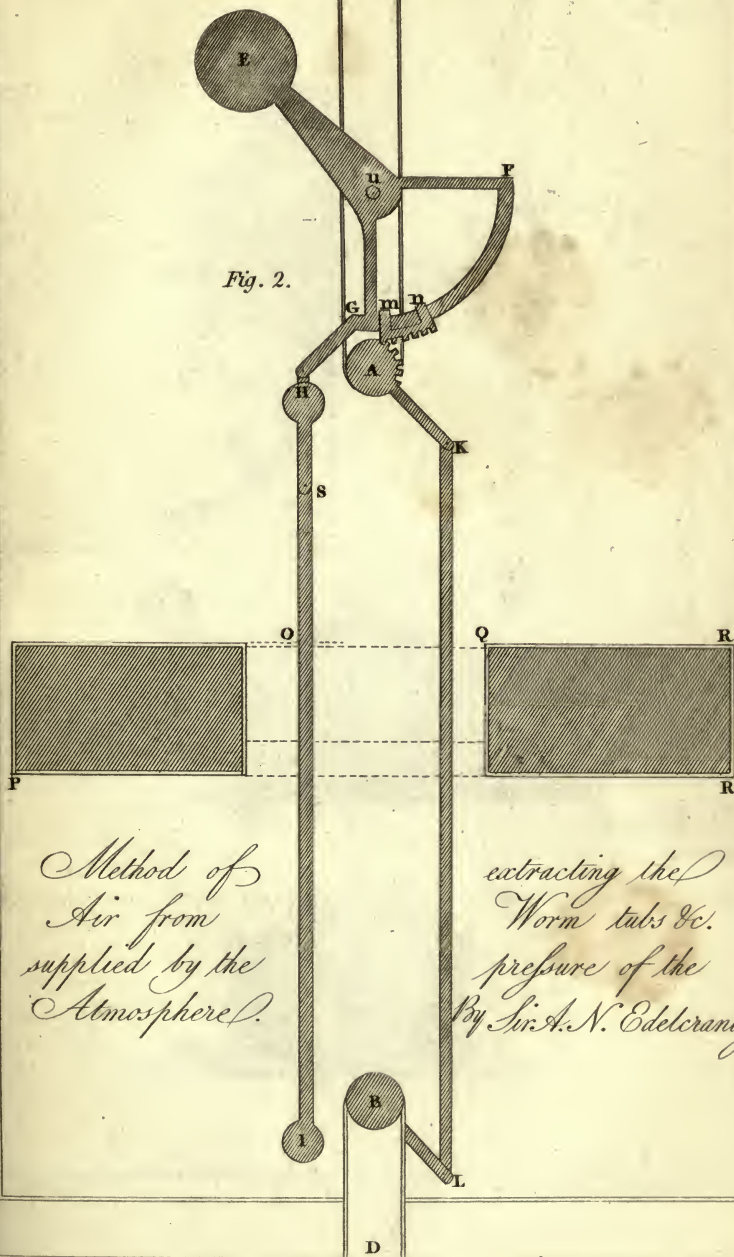






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Fig. 2.



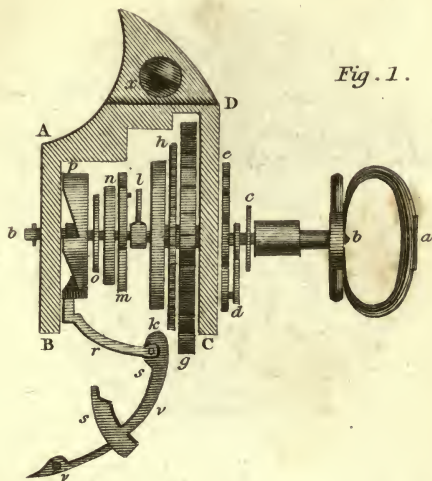
Method of  
Air from  
supplied by the  
Atmosphere.

extracting the  
Worm tubs &c.  
pressure of the  
By Sir. A. N. Edelcrantz

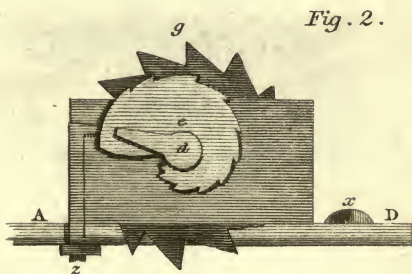




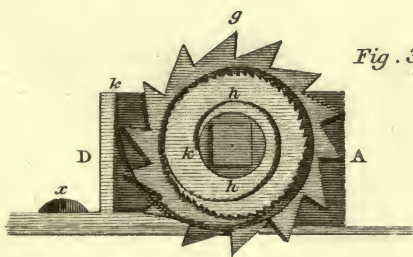
*New & Simple Repeater,  
by M<sup>r</sup>. J. M. Elliot.*



*Fig. 1.*

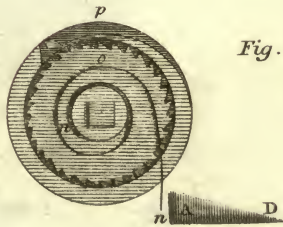
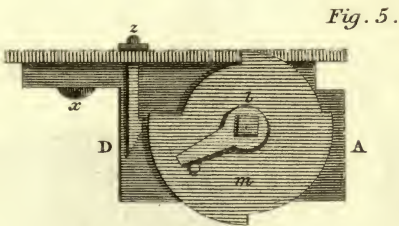
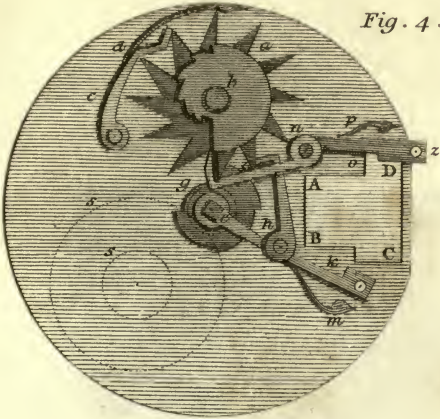


*Fig. 2.*



*Fig. 3.*

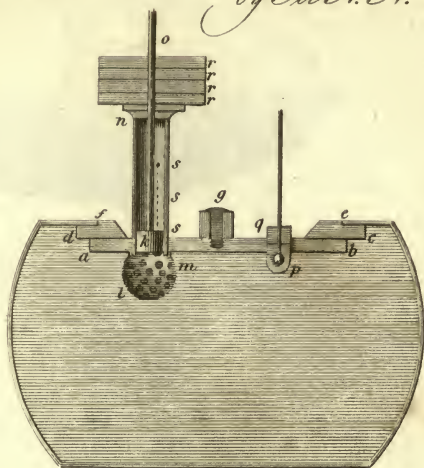




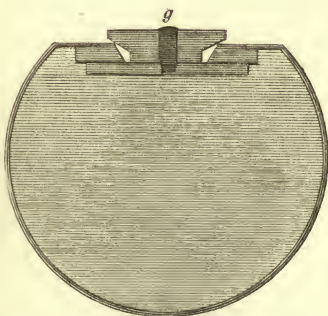




*Digester, in which the Heat,  
and pressure, are rendered stationary.  
By Sir A. N. Edelcrantz.*



*Fig. 1.*



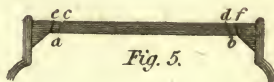
*Fig. 2.*



*Fig. 3.*



*Fig. 4.*

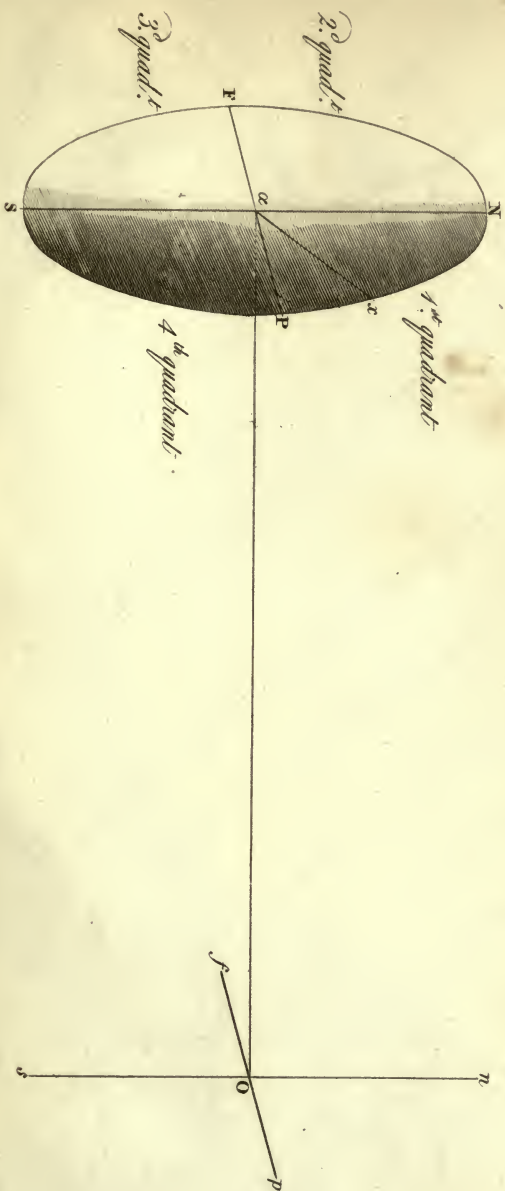


*Fig. 5.*





Dr. Hewit's theory of the Changes in the  
position of double stars.





*Remarkable Strata of Flint.*







*Mercurial Air Pump*  
By Sir A. N. Edelerantz.

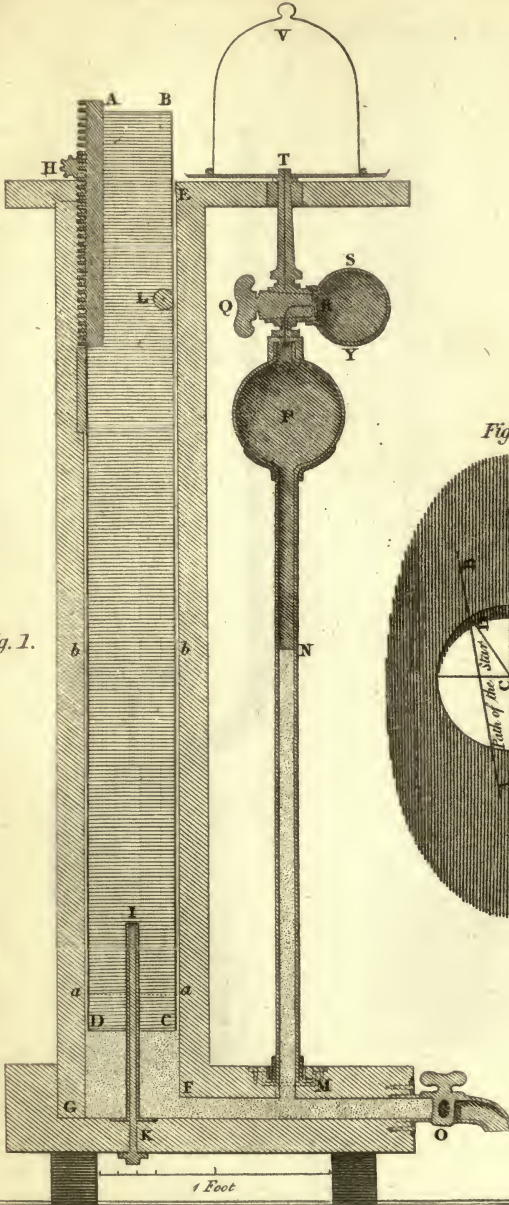
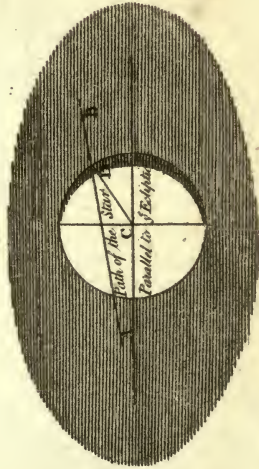


Fig. 2.





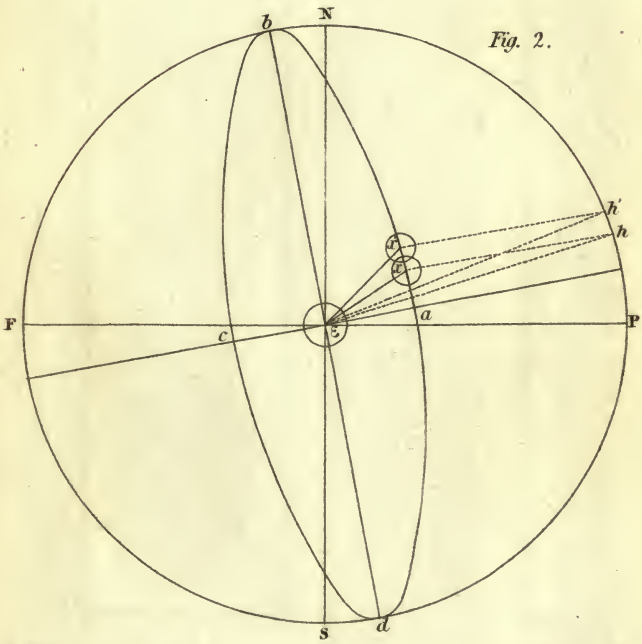
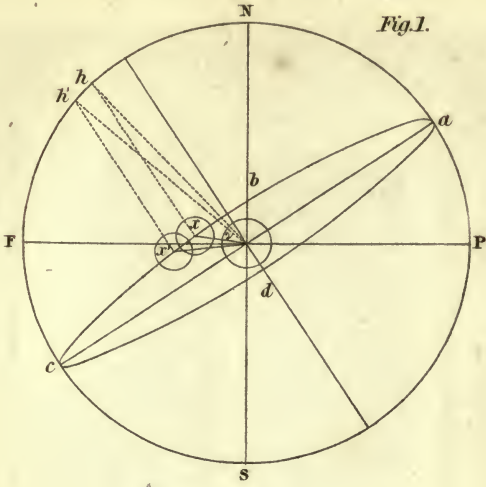


*Apparatus for Cleansing Chimnies,  
by M.<sup>r</sup> J. C. Hornblower.*





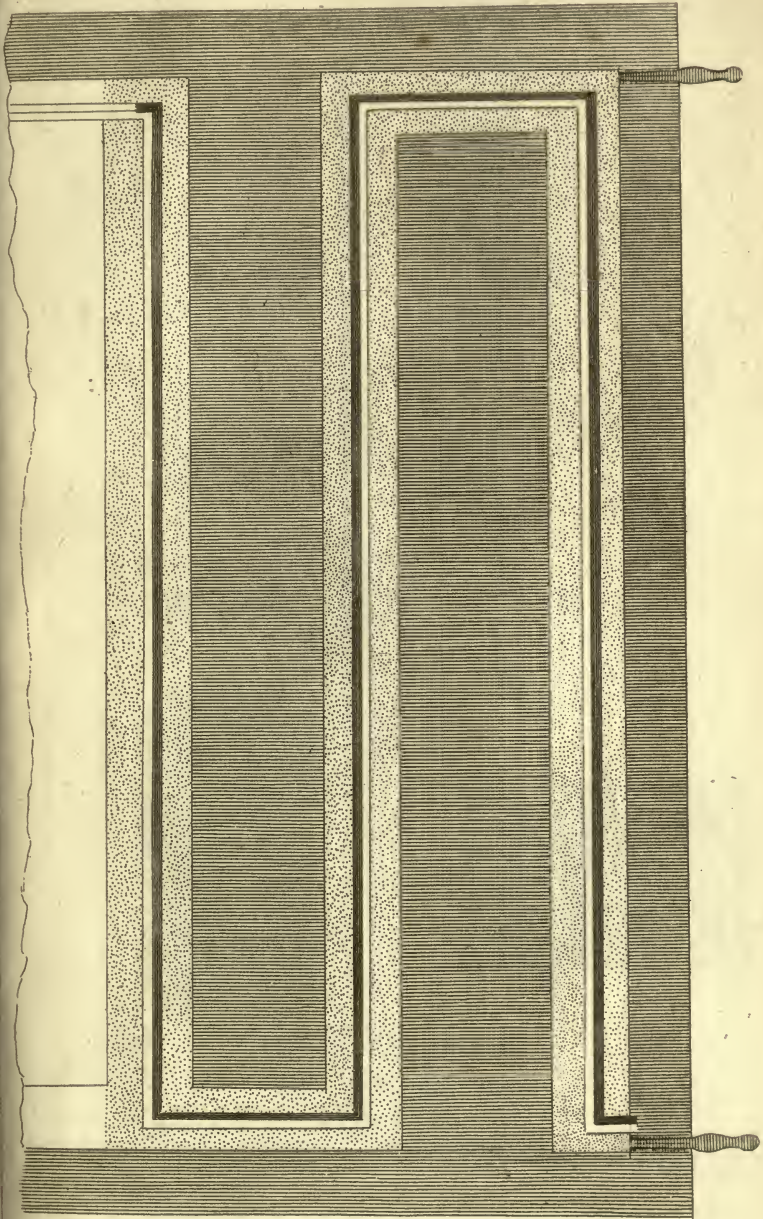
*D.<sup>r</sup> Herschel's, Investigation of the Changes observed in double Stars.*







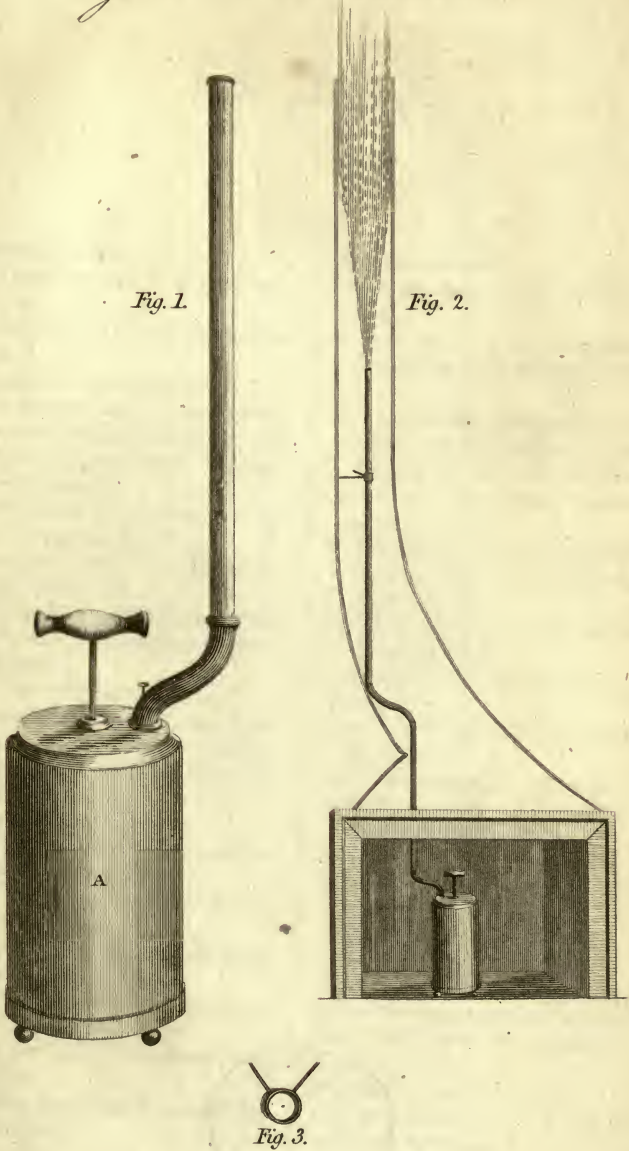
*New Galvanic Apparatus for  
combustion of Metals.*







*Apparatus for Cleansing Chimnies,  
by M<sup>r</sup>. J. C. Hornblower.*





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